PROCEEDINGS

OF THE

ROYAL SOCIETY OF EDINBURGH.

VOL. VII. 1869–70. No. 81.

Eighty-Seventh Session.

Monday, 7th February 1870.

Dr CHRISTISON, President, in the Chair.

The following Communications were read:—

1. On Reciprocal Figures, Frames, and Diagrams of Forces. By J. Clerk Maxwell, Esq., F.R.SS. L. & E.

The reciprocal figures treated of in this paper are plane rectilinear figures, such that every line in one figure is perpendicular to the corresponding line in the other, and lines which meet in a point in one figure correspond to lines which form a closed polygon in the other.

By turning one of the figures round 90°, the corresponding lines become parallel, and are more easily recognised. The practical use of these figures depends on the proposition known as the "Polygon of Forces." If we suppose one of the reciprocal figures to represent a system of points acted on by tensions or pressures along the lines of the figure, then, if the forces which act along these lines are represented in magnitude, as they are in direction, by the corresponding lines of the other reciprocal figure, every point of the first figure will be in equilibrium. For the forces which act at that point are parallel and proportional to the sides of a polygon formed by the corresponding lines in the other figure.

In all cases, therefore, in which one of the figures represents a frame, or the skeleton of a structure which is in equilibrium under the action of pressures and tensions in its several pieces, the other figure represents a system of forces which would keep the frame in equilibrium; and, if the known data are sufficient to determine these forces, the reciprocal figure may be drawn so as to represent, on a selected scale, the actual values of all these forces.

In this way a practical method of determining the tensions and pressures in structures has been developed. The "polygon of forces" has been long known. The application to polygonal frames, with a system of forces acting on the angles, and to several other cases, was made by Professor Rankine in his Applied Mechanics. Mr W. P. Taylor, a practical draughtsman, has independently worked out more extensive applications of the method. Starting from Professor Rankine's examples, I taught the method to the class of Applied Mechanics in King's College, London, and published a short account of it in the "Philosophical Magazine" for April 1864. Professor Fleeming Jenkin, in a paper recently presented to the Society, has fully explained the application of the method to the most important cases occurring in practice, and I believe that it has been found to have three important practical advantages. It is easily taught to any person who can use a ruler and scale. It is quite sufficiently accurate for all ordinary calculations, and is much more rapid than the trigonometrical method. When the figure is drawn the whole process remains visible, so that the accuracy of the drawing of any single line can be afterwards tested; and if any mistake has been made, the figure cannot be completed. Hence the verification of the process is much easier than that of a long series of arithmetical operations, including the use of trigonometric tables.

In the present paper I have endeavoured to develope the idea of reciprocal figures, to show its connection with the idea of reciprocal polars as given in pure mathematics, and to extend it to figures in three dimensions, and to cases in which the stresses, instead of being along certain lines only, are distributed continuously throughout the interior of a solid body. In making this extension of the theory of reciprocal figures, I have been led to see the connection of this theory with that of the very important function introduced into the theory of stress in two dimensions by Mr Airy, in his paper "On the Strains in the Interior of Beams" (Phil. Trans. 1863).

If a plane sheet is in equilibrium under the action of internal stress of any kind, then a quantity, which we shall call Airy's Function of Stress, can always be found, which has the following properties.

At each point of the sheet let a perpendicular be erected proportional to the function of stress at that point, so that the extremities of such perpendiculars lie in a certain surface, which we may call the surface of stress. In the case of a plane frame the surface of stress is a plane-faced polyhedron, of which the frame is the projection. On another plane, parallel to the sheet, let a perpendicular be erected of height unity, and from the extremity of this perpendicular let a line be drawn normal to the tangent plane at a point of the surface of stress, and meeting the plane at a certain point.

Thus, if points be taken in the plane sheet, corresponding points may be found by this process in the other plane, and if both points are supposed to move, two corresponding lines will be drawn, which have the following property:—The resultant of the whole stress exerted by the part of the sheet on the right hand side of the line on the left hand side, is represented in direction and magnitude by the line joining the extremities of the corresponding line in the other figure. In the case of a plane frame, the corresponding figure is the reciprocal diagram described above.

From this property the whole theory of the distribution of stress in equilibrium in two dimensions may be deduced.

In the most general case of three dimensions, we must use three such functions, and the method becomes cumbrous. I have, however, used these functions in forming equations of equilibrium of elastic solids, in which the stresses are considered as the quantities to be determined, instead of the displacements, as in the ordinary form.

These equations are especially useful in the cases in which we wish to determine the stresses in uniform beams. The distribution of stress in such cases is determined, as in all other cases, by the elastic yielding of the material; but if this yielding is small and the beam uniform, the stress at any point will be the same, whatever be the actual value of the elasticity of the substance.

Hence the coefficients of elasticity disappear from the ultimate values of the stresses.

In this way I have obtained values for the stresses in a beam supported in a specified way, which differ only by small quantities from the values obtained by Mr Airy, by a method involving certain assumptions, which were introduced in order to avoid the consideration of elastic yielding.

2. On the Extension of Brouncker's Method.

By Edward Sang, Esq.

The operation in use by the ancient geometers for finding the numerical expression for the ratio of two quantities, was to repeat each of them until some multiple of the one agreed with a multiple of the other; the numbers of the repetitions being inversely proportional to the magnitudes.

The modern process, introduced by Lord Brouncker, under the name of continued fractions, is to seek for that submultiple of the one which may be contained exactly in the other; the numbers being then directly proportional to the quantities compared.

On applying this method to the roots of quadratic equations, the integer parts of the denominators were found to recur in periods; and Lagrange showed that, while all irrational roots of quadratics give recurring chain-fractions, every recurring chain-fraction expresses the root of a quadratic; and hence it was argued that this phenomenon of recurrence is exhibited by quadratic equations alone.

The author of this paper had supplemented Lagrange's proposition, by showing that when the progression of fractions converging to one root of a quadratic is continued backwards, the convergence is toward the other root. The singularity of this exclusive property of quadratic equations led him to consider whether some analogous property may not be possessed by equations of higher degrees. Putting aside the idea of the chain-fraction as being merely accidental to the subject, and attending to the series of converging fractions, he came upon a kind of recurrence which extends to equations of all orders; and which proceeds by operating on two, three, or more contiguous terms according to the rank of the equation. In this way a ready means of approximating to the greatest and to the least root of any equation was obtained.

The following cases were cited:—

If we begin with the terms $\frac{1}{0}$, $\frac{1}{1}$, and form a progression by adding the respective members of the preceding term to the doubles of those of the last, thus—

$$\frac{1}{0}, \frac{1}{1}, \frac{3}{2}, \frac{7}{5}, \frac{17}{12}, \frac{41}{29}, \frac{99}{70}, \frac{239}{169}, \text{ \&c.}$$

we form the well-known series converging to the ratio of the diagonal of a square to the side.

Beginning with the terms 0, 1, if we add together the last two, thus—

0, 1, 1, 2, 3, 5, 8, 13, 21, 34, 55, 89, &c.,

each term bears to the succeeding one a ratio approaching to that of the side of a regular pentagon to the diagonal thereof.

If we assume the three terms 0, 0, 1, and continue the progression by adding to the double of the last term, the difference of the two preceding ones, thus—

0, 0, 1, 2, 5, 11, 25, 56, 126, 283, 636, 1429, &c.,

the ratio of each term to the following approaches to that of the side to the greater diagonal of a regular heptagon.

Or again, beginning with the same three terms, if we form a progression by deducting the antepenult from the triple of the last term, thus—

0, 0, 1, 3, 9, 26, 75, 216, 622, 1791, 5157, &c.,

we obtain an approximation to the ratio of the side to the longest diagonal of a regular enneagon.

From these examples it would appear that important results may be expected from the study of this branch of Logistics. Now, the roots of quadratics were reached by the comparison of two magnitudes, wherefore those of cubics may result from the comparison of three incommensurables; and analogously for equations of higher degrees. The comparison of several magnitudes thus forms the subject of the paper.

Assuming three homogeneous quantities, A, B, C, arranged in the order of their magnitudes, we take the second B as often as possible from the greatest A, and obtain a remainder less than B; this remainder may or may not be greater than C. If it be greater, we take C as often as possible from it, and obtain a remainder D less than C, the least of the three quantities. B, C, D may now be treated in the same way, and thus we form a series of equations—

$$\begin{aligned} A &= p_1B + q_1C + D \\ B &= p_2C + q_2D + E \\ C &= p_3D + q_3E + F, \text{ \&c.,} \end{aligned}$$

in which p can never be zero, while q may be so.

In order to compute, by help of these quotients, the approximate ratios of A, B, C, we may put A_1, A_2, A_3, &c.; B_1, B_2, B_3, &c.; C_1, C_2, C_3, &c., for the corresponding successive values, and then we obtain the equations—

$$\begin{aligned} A_{n+1} &= p_{n+1}\,A_n + q_n\,A_{n-1} + A_{n-2}, \\ B_{n+1} &= p_{n+1}\,B_n + q_n\,B_{n-1} + B_{n-2}, \\ C_{n+1} &= p_{n+1}\,C_n + q_n\,C_{n-1} + C_{n-2}, \end{aligned}$$

which indicate a very simple arrangement, best studied from an example. Thus, if the successive equations were—

$$\begin{aligned} A &= 2.B + 1.C + D \\ B &= 3.C + 2.D + E \\ C &= 2.D + 0.E + F \\ D &= 3.E + 1.F + G \\ E &= 2.F + 2.G + H \\ F &= 3.G + 0.H + I \\ G &= 2.H + 1.I + K \\ H &= 3.I + 2.K + L, \text{ \&c.} \end{aligned}$$

we should write the values of p, q, 1 in horizontal lines as in the accompanying scheme; and the successive approximate values of A, B, C in lines below them. Unit being written as the first value of A under p_1, which in this case is 2, we multiply this by 2, and

	1	1	1	1	1	1	1	1	1	
2	1	2	0	1	2	0	1	2	0	
p	2	3	2	3	2	3	2	3	2	
A	1	2	7	19	59	144	569	1197	4304	11571
B	...	1	3	8	25	61	241	507	1823	4901
C	...	...	1	2	6	15	59	124	446	1199
D	...	...	...	1	3	7	28	59	212	570
E	...	...	...	...	1	2	8	17	61	164
F	...	...	...	...	...	1	3	6	22	59
G	...	...	...	...	...	...	1	2	7	19
H	...	...	...	...	...	...	...	1	3	8
I	...	...	...	...	...	...	...	...	1	2
K	...	...	...	...	...	...	...	...	...	1

write the product in the column containing p_2, q_2. We then multiply the newly found A by the p above it; the preceding A by its q, that is in this case 3.2 and 1.1, and write the sum 7 as the third value of A. Again, taking the sum of the products p_3A_3, q_2A_2, and, as we may call it for generality's sake, r_1A_1, we have $2.7 + 2.2 + 1.1 = 19$ for A_4. In this way we obtain the successive values of A.

The values of B are found in the same way, observing that $B_1 = 0$, $B_2 = 1$. So also are the values of C, and if it be wished, those of D, E, F, &c., obtained, the first effective term being delayed a step, as shown in the scheme.

This method was applied to the three irrational quantities, log 5, log 3, and log 2; and the results were used in explaining the doctrine of musical temperaments.

When two quantities only are compared, it is well known that the cross products of the adjoining fractions differ by unit, or that, taking three contiguous terms, such as—

$$\frac{A_3}{B_3}, \frac{A_4}{B_4}, \frac{A_5}{B_5}, \text{ we have the equation,}$$

$$A_3B_4 - A_4B_3 = - A_4B_5 + A_5B_4,$$

which may be expressed, according to Cayley's notation of determinants—

$$\begin{vmatrix} A_3 & A_4 \\ B_3 & B_4 \end{vmatrix} = - \begin{vmatrix} A_4 & A_5 \\ B_4 & B_5 \end{vmatrix} = \pm 1.$$

In the very same way, when three magnitudes are compared, we have the equation—

$$\begin{vmatrix} A_3 & A_4 & A_5 \\ B_3 & B_4 & B_5 \\ C_3 & C_4 & C_5 \end{vmatrix} = + \begin{vmatrix} A_4 & A_5 & A_6 \\ B_4 & B_5 & B_6 \\ C_4 & C_5 & C_6 \end{vmatrix} = + 1.$$

that is to say, this determinant is unit throughout.

The extension of this method to more than three quantities is easy. In conclusion, an opinion was expressed, that as the Brounckerian process applied to two magnitudes has already thrown great light on the doctrine of squares, this extension of it may be expected to do as much for the still higher departments of the theory of numbers.

3. On the Forces experienced by Solids immersed in a Moving Liquid. By Sir William Thomson.

Cyclic irrotational motion,* [§ 60 (z)] once established through an aperture or apertures, in a movable solid immersed in a liquid, continues for ever after with circulation or circulations unchanged, [§ 60 (a)] however the solid be moved, or bent, and whatever influences experienced from other bodies. The solid, if rigid and left at rest, must clearly continue at rest relatively to the fluid surrounding it to an infinite distance, provided there be no other solid within an infinite distance from it. But if there be any other solid or solids at rest within any finite distance from the first, there will be mutual forces between them, which, if not balanced by proper application of force, will cause them to move. The theory of the equilibrium of rigid bodies in these circumstances might be called Kinetico-statics; but it is in reality a branch of physical statics simply. For we know of no case of true statics in which some if not all of the forces are not due to motion; whether as in the case of the hydrostatics of gases, thanks to Clausius and Maxwell, we perfectly understand the character of the motion, or, as in the statics of liquids and elastic solids, we only know that some kind of molecular motion is essentially concerned. The theorems which I now propose to bring before the Royal Society regarding the forces experienced by bodies mutually influencing one another through the mediation of a moving liquid, though they are but theorems of abstract hydrokinetics, are of some interest in physics as illustrating the great question of the 18th and 19th centuries:—Is action at a distance a reality, or is gravitation to be explained, as we now believe magnetic and electric forces must be, by action of intervening matter?

I. (Proposition.) Consider first a single fixed body with one or more apertures through it; as a particular example, a piece of straight tube open at each end. Let there be irrotational circulation of the fluid through one or more such apertures. It is readily proved [from § 63 *Exam.* (2.)]* that the velocity of the fluid at any point in the neighbourhood agrees in magnitude and direction with the resultant electro-magnetic force, at the corresponding point, in the neighbourhood of an electro-magnet replacing the solid, constructed according to the following specification. The "core," on which the "wire" is wound, is to be of any material having infinite diamagnetic inductive capacity,† and is to be of the same size and shape as the solid immersed in the fluid. The wire is to form an infinitely thin layer or layers, with one circuit going round each aperture. The whole strength of current in each circuit, reckoned in absolute electro-magnetic measure, is to be equal to the circulation of the fluid through that aperture divided by $\sqrt{4\pi}$. The resultant electro-magnetic force at any point will be numerically equal to the resultant fluid velocity at the corresponding point in the hydrokinetic system, multiplied by $\sqrt{4\pi}$.

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* The references §§ without farther title are to the author's paper on Vortex Motion, recently published in the Transactions (1869), which contains definitions of all the new terms used in the present article. Proofs of such of the propositions now enunciated as require proof are to be found in a continuation of that paper.

Thus, considering, for example, the particular case of a straight tube open at each end, let the diameter be infinitely small in comparison with the length. The "circulation" will exceed by but an infinitely small quantity the product of the velocity within the tube into the length. In the neighbourhood of each end, at distances from it great in comparison with the diameter of the tube and short in comparison with the length, the stream lines will be straight lines radiating from the end. The velocity, outwards from one end and inwards towards the other, will therefore be inversely as the square of the distance from the end. Generally at all considerable distances from the ends, the distribution of fluid velocity will be the same as that of the magnetic force in the neighbourhood of an infinitely thin bar longitudinally magnetised uniformly from end to end.

Merely as regards the comparison between fluid velocity and resultant magnetic forces, Euler's fanciful theory of magnetism is thus curiously illustrated. This comparison, which has been long known as part of the correlation between the mathematical theories of electricity, magnetism, conduction of heat, and hydrokinetics, is merely kinematical, not dynamical. When we pass, as we presently shall, to a strictly dynamical comparison relatively to the mutual force between two hard steel magnets, we shall find the same law of mutual action between two tubes, with liquid flowing through each, but with this remarkable difference, that the forces are opposite in the two cases; unlike poles attracting and like poles repelling in the magnetic system, while in the hydrokinetic there is attraction between like ends and repulsion between unlike ends.

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* Or from Helmholtz's original integration of the hydrokinetic equations.

† Real diamagnetic substances are, according to Faraday's very expressive language, relatively to lines of magnetic force, *worse conductors* than air.

The ideal substance of infinite diamagnetic inductive capacity is a substance which completely *sheds off* lines of magnetic force, or which is perfectly impervious to *magnetic force.*

II. (Proposition.) Consider two or more fixed bodies, such as the one described in Prop. I. The mutual actions of two of these bodies are equal, but in opposite directions, to those between the corresponding electro-magnets. The particular instance referred to above shows us the remarkable result, that through fluid pressure we can have a system of mutual action, in which like attracts like with force varying inversely as the square of the distance. Thus, if the exit ends of tubes, open at each end with fluid flowing through them, be placed in the neighbourhood of one another, and the entering ends be at infinite distances, the mutual forces resulting will be simply attractions according to this law. The lengths of the tubes on this supposition are infinitely great, and therefore, as is easily proved from the conservation of energy, the quantities flowing out per unit of time are but infinitesimally affected by the mutual influence.

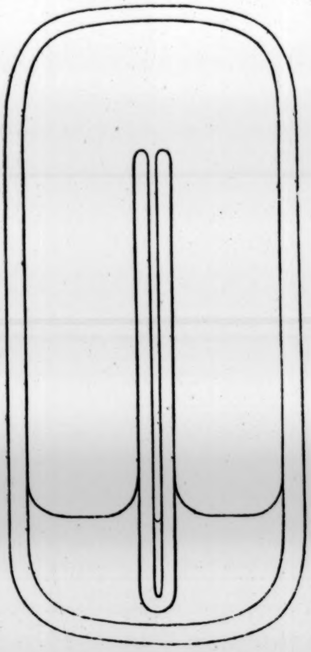
III. Proposition II. holds, even if one of the bodies considered be merely a solid, with or without apertures; if with apertures, having no circulation through them. In such a case as this the corresponding magnetic system consists of a magnet or electro-magnet, and a merely diamagnetic body, not itself a magnet, but disturbing the distribution of magnetic force around it by its diamagnetic influence. Thus, for example, a spherical solid at rest in the field of motion surrounding a fixed body, through apertures in which there is cyclic irrotational motion, will experience from fluid pressure a resultant force through its centre equal and opposite to that experienced by a sphere of infinite diamagnetic capacity, similarly situated in the neighbourhood of the corresponding electro-magnet. Therefore, according to Faraday's law for the latter, and the comparison asserted in Prop. I., it would experience a force from places of less towards places of greater fluid velocity, irrespectively of the direction of the stream lines in its neighbourhood; a result easily deduced from the elementary formula for fluid pressure in hydrokinetics.

I have long ago shown that an elongated diamagnetic body in a uniform magnetic field tends, as tends an elongated ferromagnetic body, to place its length along the lines of force. Hence a long solid, pivoted on a fixed axis through its middle in a uniform stream of liquid, tends to place its length perpendicularly across the direction of motion; a known result (Thomson & Tait's "Natural Philosophy," § 335). Again, two globes held in a uniform stream with the lines joining their centres, require force to prevent them from mutually approaching one another. In the magnetic analogue, two spheres of diamagnetic or ferromagnetic inductive capacity repel one another when held in a line at right angles to the lines of force. A hydrokinetic result similar to this for the case of two equal globes, is to be found in Thomson and Tait's "Natural Philosophy," § 332.

IV. (Proposition.) If the second body considered in § III., that is to say, a body either having no apertures, or, if perforated, having no circulation through the apertures, be acted on by one system of forces applied so as always to balance the resultant of the fluid pressure, calculated for it according to II. and III. for whatever position it may come to at any time, and if it be influenced, besides, by any other system of applied forces, superimposed on the former, it will move just as it would move, under the influence of the latter system of forces alone, were the fluid at rest, except in so far as compelled to move by the body's own motion through it. A particular case of this proposition was first published many years ago, by Professor James Thomson, on account of which he gave the name of "vortex of free mobility" to the cyclic irrotational motion symmetrical round a straight axis.

4. On the Equilibrium of Vapour at a Curved Surface of Liquid. By Sir William Thomson.

In a closed vessel containing only a liquid and its vapour, all at one temperature, the liquid rests, with its free surface raised or depressed in capillary tubes and in the neighbourhood of the solid boundary, in permanent equilibrium according to the same law of relation between curvature and pressure as in vessels open to the air. The permanence of this equilibrium implies physical equilibrium between the liquid and the vapour in contact with it at all parts of its surface. But the pressure of the vapour at different levels differs according to hydrostatic law. Hence the pressure of saturated vapour in contact with a liquid differs according to the curvature of the bounding surface, being less when the liquid is concave, and greater when it is convex. And detached portions of the liquid in separate vessels all enclosed in one containing vessel, cannot remain permanently with their free surfaces in any other relative positions than those they would occupy if there were hydrostatic communication of pressure between the portions of liquid in the several vessels. There must be evaporation from those surfaces which are too high, and condensation into the liquid at those surfaces which are too low—a process which goes on until hydrostatic equilibrium, as if with free communication of pressure from vessel to vessel, is attained. Thus, for example, if there are two large open vessels of water, one considerably above the other in level, and if the temperature of the surrounding matter is kept rigorously constant, the liquid in the higher vessel will gradually evaporate until it is all gone and condensed into the lower vessel. Or if, as illustrated by the annexed diagram, a capillary tube, with a small quantity of liquid occupying it from its bottom up to a certain level, be placed in the neighbourhood of a quantity of the same liquid with a wide free surface, vapour will gradually become condensed into the liquid in the capillary tube until the level of the liquid in it is the same as it would be were the lower end of the tube in hydrostatic communication with the large mass of liquid. Whether air be present above the free surface of the liquid in the several vessels or not, the condition of ultimate equilibrium is the same; but the processes of evaporation and condensation through which equilibrium is approached will be very much retarded by the presence of air. The experiments of Graham, and the kinetic theory of Clausius and Maxwell, scarcely yet afford us sufficient data for estimating the rapidity with which the vapour proceeding from one of the liquids will diffuse itself through the air and reach the surface of another liquid at a lower level. With air at anything approaching to ordinary atmospheric density to resist the process, it is probable it would be too slow to show any results unless in very long continued experiments. But if the air be removed as perfectly as can be done by well-known practical methods, it is probable that the process will be very rapid: it would, indeed, be instantaneous, were it not for the cold of evaporation in one vessel and the heat of condensation in the other. Practically, then, the rapidity of the process towards hydrostatic equilibrium through vapour between detached liquids, depends on the rate of the conduction of heat between the several surfaces through intervening solids and liquids. Without having made either the experiment, or any calculations on the rate of conduction of heat in the circumstances, I feel convinced that in a very short time water would visibly rise in the capillary tube indicated in the diagram, and that, provided care is taken to maintain equality of temperature all over the surface of the hermetically sealed vessel, the liquid in the capillary tube would soon take very nearly the same level as it would have were its lower end open; sinking to this level if the capillary tube were in the beginning filled too full, or rising to it if (as indicated in the diagram) there is not enough of liquid in it at first to fulfil the condition of equilibrium.



The following formulæ show precisely the relations between curvatures, differences of level, and differences of pressure, with which we are concerned.

Let ρ be the density of the liquid, and σ that of the vapour; and let T be the cohesive tension of the free surface, per unit of breadth, in terms of weight of unit mass, as unit of force. Let h denote the height of any point, P, of the free surface above a certain plane of reference, which I shall call for brevity the plane level of the free surface. This will be sensibly the actual level of the free surface in regions, if there are any, with no part of the edge (or bounding line of the free surface where liquid ends and solid begins) at a less distance than several centimetres. Lastly, let r and r' be the principal radii of curvature of the surface at P. By Laplace's well-known law, we have, as the equation of equilibrium,

$$(\rho - \sigma)h = \mathrm{T}\left(\frac{1}{r} + \frac{1}{r'}\right) \quad . \quad . \quad . \quad (1).$$

Now, in the space occupied by vapour, the pressure is less at the higher than at the lower of two points whose difference of levels is h, by a difference equal to σh. And there is permanent equilibrium between vapour and liquid at all points of the free surface. Hence the pressure of vapour in equilibrium is less at a concave than at a plane surface of liquid, and less at a plane surface than at a convex surface, by differences amounting to $\frac{\mathrm{T}\sigma}{\rho - \sigma}$ per unit difference of curvature. That is to say, if ϖ denote the pressure of vapour in equilibrium at a plane surface of liquid, and p the pressure of vapour of the same liquid at the same temperature presenting a curved surface to the vapour, we have

$$p = \varpi - \frac{\mathrm{T}\sigma}{\rho - \sigma}\left(\frac{1}{r} + \frac{1}{r'}\right) \quad . \quad . \quad (2),$$

$\frac{1}{r}$ and $\frac{1}{r'}$ being the curvatures in the principal sections of the surface bounding liquid and vapour, reckoned positive when concave towards the vapour.

In strictness, the value of σ to be used in these equations, (1) and (2), ought to be the mean density of a vertical column of vapour, extending through the height h from the plane of reference.

But in all cases to which we can practically apply the formulæ, according to present knowledge of the properties of matter, the difference of densities in this column is very small, and may be neglected. Hence, if H denote the height of an imaginary homogeneous fluid above the plane of reference, which, if of the same density as the vapour at that plane, would produce by its weight the actual pressure ϖ, we have

$$\sigma = \frac{\varpi}{H} .$$

Hence by (1) and (2)

$$p = \varpi \left(1 - \frac{h}{H}\right) \quad . \quad . \quad . \quad (3).$$

For vapour of water at ordinary atmospheric temperatures, H is about 1,300,000 centimetres. Hence, in a capillary tube which would keep water up to a height of 13 metres above the plane level, the curved surface of the water is in equilibrium with the vapour in contact with it, when the pressure of the vapour is less by about $\frac{1}{1000}$th of its own amount than the pressure of vapour in equilibrium at a plane surface of water at the same temperature.

For water the value of T at ordinary temperatures is about ·08 of a gramme weight per centimetre; and ρ, being the mean of a cubic centimetre, in grammes, is unity. The value of σ for vapour of water, at any atmospheric temperature, is so small that we may neglect it altogether in equation (1). In a capillary tube thoroughly wet with water, the free surface is sensibly hemispherical, and therefore r and r' are each equal to the radius of the inner surface of the liquid film lining the tube above the free liquid surface; we have, therefore,

$$h = \cdot 08 \times \frac{2}{r} .$$

Hence, if $h = 1300$ centimetres, $r = \cdot 00012$ centimetres. There can be no doubt but that Laplace's theory is applicable without serious modification even to a case in which the curvature is so great (or radius of curvature so small) as this. But in the present state of our knowledge we are not entitled to push it much further. The molecular forces assumed in Laplace's theory to be "insensible at sensible distances," are certainly but little, if at all, sensible at distances equal to or exceeding the wave lengths of ordinary light. This is directly proved by the most cursory observation of soap bubbles. But the appearances presented by the black spot which abruptly ends the series of colours at places where the bubble is thinnest before it breaks, make it quite certain that the action of those forces becomes sensible at distances not much less than a half wave length, or $\frac{1}{40000}$ of a centimetre. There is, indeed, much and multifarious evidence that in ordinary solids and liquids, not merely the distances of sensible inter-molecular action, but the linear dimensions of the molecules themselves, and the average distance from centre to nearest centre,* are but very moderately small in comparison with the wave lengths of light. Some approach to a definite estimate of the dimensions of molecules is deducible from Clausius' theory of the average spaces travelled without collision by molecules of gases, and Maxwell's theory and experiments regarding the viscosity of gases. Having perfect confidence in the substantial reality of the views which these grand investigations have opened to us, I find it scarcely possible to admit that there can be as many as 10^{27} molecules in a cubic centimetre of liquid carbonic acid or of water. This makes the average distance from centre to nearest centre in the liquids exceed a thousand-millionth of a centimetre!

We cannot, then, admit that the *formulæ* which I have given above are applicable to express the law of equilibrium between the moisture retained by vegetable substances, such as cotton cloth or oatmeal, or wheat-flour biscuits, at temperatures far above the dew point of the surrounding atmosphere. But although the energy of the attraction of some of these substances for vapour of water (when, for example, oatmeal, previously dried at a high temperature, has been used, as in the original experiment of Sir J. Leslie, to produce the freezing of water under the receiver of an air-pump), is so great that it might almost claim recognition from chemists as due to a "chemical affinity," and resulting in a "chemical combination," I believe that the absorption of vapour into fibrous and cellular organic structures is a property of matter continuous with the absorption of vapour into a capillary tube demonstrated above.

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* By "average distance from centre to nearest centre," I mean the side of the cube in a cubic arrangement of a number of points equal to the number of real molecules in any space.

5. On a Bow seen on the Surface of Ice. By J. Clerk Maxwell, Esq., F.R.SS. L. & E.

On the 26th of January, about noon, I observed the appearance of a coloured bow on the frozen surface of the ditch which surrounds S. John's College, Cambridge. Its appearance and position seemed to correspond with those of an ordinary primary rainbow. I at once made a rough measurement of the angle on the board of a book which I had with me, and then borrowed from Dr Parkinson, President of S. John's College, a sextant, with which I found that the angle between the bright red and the shadow of the large mirror was 41° 50′, and that for bright blue 40° 30′. The angle for the extreme red of the primary bow, as given in Parkinson's Optics, is 42° 20′, and that for violet 40° 32′. The bows formed by ice crystals are seen on the same side as the sun, and not on the opposite side. I suppose the bow which I saw to be formed by small drops of water lying on the ice. If the lower part of each drop were flattened, so as to bring the point at which the reflexion takes place nearer to the points of incidence and emergence, the effect would be of the same kind as that of a diminution of the index of refraction—that is, the angle of the bow would be increased. How a drop of water can lie upon ice without wetting it, and losing its shape altogether, I do not profess to explain.

Only a small part of the ice presented this appearance. It was best seen when the incident and emergent rays were nearly equally inclined to the horizontal. The ice was very thin, and I was not able to get near enough to the place where the bow appeared to see if the supposed water drops really existed.

The following Gentlemen were admitted Fellows of the Society:—

W. E. Heathfield, Esq., F.R.G.S., F.C.S.
Edward James Shearman, M.D., F.R.C.S.L.
Patrick D. Swan, Esq.
Dr H. Alleyne Nicholson.

A ballot also took place for the Rev. Dr Hodson, who resigned the Fellowship of the Society in 1867. Dr Hodson was readmitted.

Monday, 21*st February* 1870.

PROFESSOR KELLAND, Vice-President, in the Chair.

The following Communications were read:—

1. Note on the Atomic Volume of Solid Substances. By James Dewar, Lecturer on Chemistry, Veterinary College, Edinburgh.

The investigation of the volume retained by different elementary substances, when combined in the solid condition, has attracted the attention of many chemists. We have only to look at the laborious memoirs of Schröter, Kopp, Playfair and Joule, Boullay, Filhol, and others, to be convinced of the great amount of labour expended on the subject. Nor is it at all remarkable that so many workers should take to this field of research, when we remember the simplicity of the laws regulating the combining volumes of gaseous substances, and the probable extension of some such similar law to the solid condition of matter. Emboldened by analogy, the forementioned workers endeavoured to find some constant to which volumes of elements and compounds held the relation of some simple multiple, and thus extend the apparent simplicity of Prout's law of combining weights to combining volumes. The great object in view was evidently to extend the speculations and laws of Dalton and Gay Lussac to the volumes of solid substances, and thus to arrive at some general explanation of the results. However creditable the desire to reveal simplicity from out of the apparent chaos, no one, in examining the subject, can help arriving at the conclusion that the means employed to extract the seeming harmony from the results were purely arbitrary. It does not follow, however, that the results were fruitless, although no great generalisation was discovered. The solid state of matter is relatively far more complicated than either the liquid or gaseous conditions. The uniformity of expansion of gaseous matter, and the easy comparison of liquid substances under similar conditions, enable us to arrive at some satisfactory conclusions regarding the volume in these states: but, in examining solid matter, we have no guarantee that the substances are under similar physical conditions. We cannot, therefore, expect the same uniformity in the results; but although, strictly speaking, we may entertain grave doubt on the real value of the results, yet, in some cases, we cannot help recognising some curious analogies, especially on comparing similar classes of compounds. It is not the object of this note either to criticise or discuss the labours and speculations of others, no originality being claimed in the subject matter itself, all that is original being merely the addition of a few new analogies.

The first important discovery in the subject of atomic volumes was made by Schröter. He observed that the equivalent volume of oxygen, obtained by subtracting the volume of metal in the free state from the volume of the oxide, gave, approximately, the same value of 5·2 in the oxides of copper, zinc, cadmium, lead, mercury, iron, cobalt, and titanium. In other words, the oxygen occupied the same volume in each combination. Other classes of oxides gave a volume of twice, or half the above number. In order to arrive at the volume of the oxygen, Schröter started with the premises that the metal in the combined state occupied the same volume as the uncombined metal. Granting, for the present, that oxygen has a definite volume in combination in the oxides, it is clear that the volume obtained by difference will vary with the volume of the combined metal. The same method applied to the oxides of the less dense metals would give a negative volume to the oxygen; and in these cases we must admit condensation to have taken place in the metal itself. We may have three cases, therefore, according as the volume of the combined metal differs from that of the uncombined. If it remains the same in combination, we obtain the real volume; if it condenses, the volume is a minimum; if it expands, a maximum. Seeing that the oxygen in the dense metals has the volume 5·2, we may regard the greater and smaller volume obtained from some oxides as the result of condensation or expansion of the metal. Supposing the above volume (5·2) to exist generally in the oxides, we would have a condensation in the less dense metals in combination, approaching very nearly, in the case of potassium, sodium, and aluminium, to one-third, and in calcium, magnesium, and strontium to nearly one-half, of the volume in the free state. Thus far, then, this number would give a rough explanation in admitting condensation in many of the metals.

I have thought that it would be interesting to compare this volume with the volume of oxygen when it is combined with solid substances other than metallic, and to take a series of analogous combinations. For this purpose the chlorine family is well fitted in their respective combinations with potassium, and these with oxygen. The following table contains the best known density determinations and volumes of chloride, bromide, and iodide of potassium, compared with the densities of chlorate, bromate, and iodate.

	Sp. gr.	Volume.	Difference.	Mean vol. of O (=16)
KCl	1·977	37·6	15	5
$KClO_3$	2·326	52·6		
KBr	2·69	44	7	2·3
$KBrO_3$	3·271	51		
KI	3·0	55·3	–1·6	–0·5
KIO_3	3·979	53·7		

The total volume of the oxygen in chlorate of potash, on the supposition the chloride of potassium retains its original volume in combination, is 15; whereas it is only 7 in bromate of potash, if we allow that the bromide of potassium retains its original volume; and it appears to occupy no volume in iodate of potash, assuming that iodide of potassium maintains its original volume. The apparent disappearance of the volume of the oxygen, in changing iodide of potassium into iodate, is analogous to the apparent loss of volume of many salts in their water of hydration, the salt occupying the volume of the crystal water taken as ice, as pointed out many years ago by Playfair and Joule. It is clear that, in assuming the halogen compounds of potassium as retaining their primitive volume in their oxidised derivatives, we place these compound substances in the same position as the metals in the simple oxides. Now, we saw that in many oxides the volume of the oxygen varied, and that, in all probability, from metallic condensation taking place during the act of combination. The metals having the lowest density and the greatest atomic volume condense the most in combining. Generally speaking, if we examine the volumes of the halogen salts in the above table, it is clear that the equivalent volumes increase, chloride of potassium being 37, bromide 44, and iodide 55·3, and their relative stability diminishes. The equivalent volumes of chlorine, bromine, and iodine are identical in the liquid state; and thus the formation of the respective potassium compounds is one of the results of unequal condensation, the co-efficient of contraction in the formation of chloride of potassium being 0·46, bromide 0·29, iodide 0·23 per unit volume. Their formation is attended with the evolution of very different amounts of heat. The following table contains some of the constants found with reference to combination and solution:—

Constants of Group.

	Contraction per Unit Volume.	Total heat.	Heat of solution.	Diffusion times (relative).	Co-efficient of expansion per equivalent volume.	Specific heat per atom.
KCl	0·46	97086	3874	74·5	0·001429	12·88
KBr	0·29	85666	4522	119	0·001848	13·47
KI	0·23	72721	4847	166	0·002358	13·60

Generally speaking, the number found for bromide of potassium is nearly the mean of those attached to chloride and iodide. A similar observation has recently been made by M. Valsen in examining the equivalent capillary constants of these bodies. Looking at the atomic thermal number, there is a far greater likelihood of condensation taking place in the bromide and iodide of potassium in the combined state, than in case of chloride, seeing that it would be relatively far more difficult to condense. But neither the chlorate, bromate, nor iodate can be produced through the direct addition of oxygen to the respective halogen salt. And the chlorate, it is well known, evolves heat on giving off its oxygen, and thus necessitates an absorption of heat during combination. It is just possible that the heat produced during the decomposition is the result of the necessary expansion of volume in the chloride of potassium in combining with oxygen, and its return to its normal volume on losing it. It makes no change in volume to suppose that, in the one case, the oxygen is added as a whole to the chloride of potassium, or, in the other, that it is between the potassium and chlorine, each occupying its individual volume unchanged, but it would alter greatly the heat evolved in so doing. If oxygen combined with chloride of potassium as a whole, without any condensation taking place, the natural result would be an evolution of heat. But if the addition of the oxygen diminishes the co-efficient of contraction, as compared with that of the free compound, then we have a physical explanation of the evolution of heat on decomposition. In this case the actual work performed by the condensation of oxygen is retained in a potential form, and, therefore, reappears as heat on its decomposition. If, now, we examine the mode in which the oxygen is attached to the respective halogen compounds, we can trace, as a necessary consequence, the retention of varying amounts of energy. Chlorate, bromate, and iodate of potash are formed by a similar chemical reaction, according to the following formula of exchange, given in equivalents, the whole reaction supposed to take place in the presence of water:—

$$\begin{array}{ccccccc} \mathrm{6KO} & + & \mathrm{6Cl} & = & \mathrm{5KCl} & + & \mathrm{KClO_6} \\ 6(76238) & & & & 5(97086) & & \\ \mathrm{6KO} & + & \mathrm{6Br} & = & \mathrm{5KBr} & + & \mathrm{KBrO_6} \\ 6(76238) & & & & 5(85666) & & \\ \mathrm{6KO} & + & \mathrm{6I} & = & \mathrm{5KI} & + & \mathrm{KIO_6} \\ 6(76238) & & & & 5(72721) & & \end{array}$$

We have appended the thermal equivalents attending the formation of these bodies in a large volume of water. It will be obvious on comparing the formation of chlorate of potash, through the above reaction, that it may be the result of absorption of heat; whereas it is certain that the formation of iodate of potash must be attended with an evolution of heat, or else cold must be the result of their action. In special experiments, made with the object of determining the thermal action, neither absorption nor evolution of heat could be detected. Thus the formation of iodate of potash is attended with an evolution of heat. This would, then, accord with the easy transformation of the chlorates into iodates, or of chloric acid into iodic acid, and the easy transformation of the iodide of potassium into the iodate, through the action of permanganate of potash, seeing that we must have an evolution of heat.

The oxygen, therefore, may be assumed to be in a very different condition relatively to the other elements, or else we must suppose that it has not affected the co-efficient of contraction, certainly not to have diminished it. The author throws out this simply as a possible explanation; he is also well aware that many other explanations might be given, all, possibly, equally satisfactory. But a physical explanation, however far it may lie from the truth, seems to convey to us the clearest ideas of what may possibly take place.

There is one point connected with the subject of volumes that requires very careful attention. All bodies in combining do not unite with condensation; that is, the volume of the compound might exceed the volumes of the isolated constituents, and yet a large evolution of heat might take place during its formation. A well-known example is that of iodide of silver. Now, M. Fizeau has shown that iodide of silver contracts regularly with increase of temperature, and M. St Claire Deville has given an explanation of this anomaly. Deville believes that bodies combine at such a temperature as would be required to transform the volume of the compound to that of the sum of the volumes of its constituents in the free state. Applying this to iodide of silver, it is clear that contraction must take place, and in all similar cases where we have an increase of volume. One cannot help associating this increase of volume to a purely physical change of state, such as the change of water with expansion into ice. Now, as Sir William Thomson has proven that pressure lowers the freezing point of water, and Mousson has actually liquefied ice by enormous pressure, if the formation of a chemical compound is analogous to a physical change of state, we ought to be able by mere pressure to decompose a chemical compound, if the formation of that compound is attended with an increase of volume. No doubt, in order to get experimental proof of this fact, we must use a relatively weak chemical compound, one attended with the evolution of no great amount of heat; and the well-known experiments of Joule on the effect of pressure on amalgams, seems to confirm my anticipation. Joule has shown that the amalgams of zinc, lead, and tin are decomposed by pressure alone, and these are the amalgams produced with the least contraction of any. In order to get definite proof of the expansion, it is, of course, necessary to use the specific gravity of mercury in the solid state. Now, Joule states, as the mean of his experiments, that mercury in the solid form has the specific gravity 15·19, whereas in the above amalgams it would have the density of only 14·1. The observations of Matthiessen on the specific gravity of alloys enables us to confirm Joule's results :—

Lead Series (A. Matthiessen).

	Sp. Gr.	Calculated Sp. Gr.	$\frac{V + V'}{v}$
Pb_2Hg, . . .	11·979	12·008	1·0024
$PbHg$, . . .	12·484	12·358	0·9899
$PbHg_2$, . . .	12·815	12·734	0·9937

The specific gravity of the mercury used in calculating the mean density was 13·573. Now, seeing that there is little or no contraction, and even in one case a slight expansion, in taking the above specific gravity of mercury, the higher density of mercury given by Joule as the result of his experiments would necessarily lead to an expansion in their formation. To illustrate the effect of pressure on the composition of an amalgam, let us take Joule's experiments on the tin amalgam. The composition of this amalgam was 100 of mercury to 51·01 of tin, and the specific gravity 10·518. The effect of 5400 lbs. pressure for thirty days, changed the amalgam, so that it had ultimately the composition 100 of mercury to 384 of tin. It is natural to believe, therefore, that the effect of pressure in this case is quite analogous to the inverse change of state, when a body that has expanded in changing its state has been subjected to its influence.

In the early part of this paper we saw that the volume of oxygen in some oxides, instead of being 5·2, was sometimes double this amount, or even more. It has also been remarked, that if the metal in combining was to expand, the volume of the oxygen would appear as a maximum. This apparently large volume of the oxygen seems to belong to sub-oxides, such as sub-oxides of mercury and copper, and oxide of silver. If we suppose, now, that this large increase of volume in the oxygen is the result of an expansion in the metal in combining with the normal oxide, it is possible that mere pressure would decompose these oxides, at least in part, into metal and the higher oxide. The instability of a body of this type, such as sub-oxide of mercury, is well known, mere titration effecting the liberation of metal with formation of the higher oxide. In this way, therefore, it seems to support the argument adduced.

2. Note on Inverted Sugar. By James Dewar, Lecturer on Chemistry, Veterinary College, Edinburgh.

For some time past an animated discussion has been going on in the columns of the "Comptes Rendus de l'Académie des Sciences" between MM. Dubranfaut and Maumené regarding the nature of inverted sugar. M. Dubranfaut, many years ago, made many valuable additions to our knowledge concerning the composition and reactions of various sugars, especially in explaining the result of the action of dilute acids on cane sugar. He explained the levo-rotatory action of inverted sugar, and its rapidly varying power with the temperature, as the result of a molecule of water in reacting with a molecule of cane sugar, generating one molecule of glucose and one of lævulose. Dubranfaut believed that inverted sugar consisted of a mixture of glucose and lævulose in equal weights; and although he did not make a direct analysis of the product, yet he was justly entitled to assume that it was so constituted, seeing that, generally, it agreed with a mean of the properties of inulin sugar and dextrose.

In order to support the above view, he separated levo-glucose from the inverted sugar, through the insolubility of the lime compound, and compared its properties with pure lævulose. The decomposition would, according to Dubranfaut, be as follows :—

$$\begin{array}{ccccccc} & & & & \overset{+}{} & & \overset{-}{} \\ C_{12}H_{22}O_{11} & + & H_2O & = & C_6H_{12}O_6 & + & C_6H_{12}O_6 \\ +73{\cdot}8 & & & & +56 & & -106 \\ & & & & & (-25) & \end{array}$$

So thoroughly had his facts and explanations been accepted by chemists generally, that, up till a recent date, no one discovered any flaw in his researches, and therefore no doubt was thrown on the validity of this theory. Recently, Maumené has reinvestigated the composition of inverted sugar by analysis. He has attempted to separate the two sugars through the action of chloride of sodium. The dextro-glucose forms a well-defined crystalline compound with chloride of sodium, whereas the lævulose does not form any compound. The results obtained by this method differ greatly from theory. Instead of finding 50 per cent. of lævulose, he found 88 per cent. In repeating the experiments of Dubranfaut on the separation of levo-glucose by hydrate of lime, he has not met with any better results; in fact, his results are quite opposed to those of Dubranfaut.

Apart altogether from expressing an opinion on the merits of the views entertained by the different parties to this discussion, the author has thought some observations of the same subject might not be unworthy of notice at the present time.

Linneman, many years ago, applied the process of hydrogenation to the sugars that he had found so successful in treating the simple organic substances. In the way named he obtained mannite from inverted sugar, the following reaction taking place :—

$$C_6H_{12}O_6 + H_2 = C_6H_{14}O_6 .$$

Mannite had long been known to be the product of certain kinds of fermentation, and occurring as a secondary product in the vinous fermentation; but it was this elegant synthesis of Linneman that first clearly showed the connection. But although inverted sugar can be changed into mannite, the next point that demands a solution is the proving the inverted sugar to be composed of equal quantities of dextrose and lævulose. Are they both transformed by hydrogenation into mannite? or is only one of them, and which? Linneman seems to have directed his attention to the solution of this question. He states that it is only the lævulose that is so affected. The reasons why he entertains the above views are not given. In all likelihood he thought that, just as Berthelot had changed mannite by a peculiar fermentation into levo-glucose, so would the levo-glucose in inverted sugar be hydrogenised into mannite.

In repeating the action of sodium amalgam on inverted sugar, I have not seen any reason why the one sugar any more than the other should be supposed to generate the mannite. The following is a description of the mode by which the sugar was inverted and hydrogenised:—Twenty grammes of cane sugar were dissolved in 150 grms. of water, and inverted through the action of 2 grms. of sulphuric acid, keeping the solution at the temperature of 70° C., afterwards adding pure carbonate of barium, filtering, and then adding one gramme of sodium in the form of a weak amalgam. The action took place without any evolution of hydrogen. If the amalgam was impure, from the presence of other metals, it evolved hydrogen at once, and the solution became brown; otherwise it remained perfectly clear. After one month the solution gave no trace of sugar with the alkaline copper solution. It was then carefully neutralised with dilute sulphuric acid, evaporated on the water bath, the greater part of the sulphate of sodium separated by crystallisation, and the residue treated with boiling 70 per cent. alcohol, the solution filtered, and allowed to crystallise. Sometimes the mannite did not crystallise until all the alcohol had evaporated, leaving a syrup that slowly assumed the crystalline form. The product had no rotatory power. In no case was the sugar entirely changed into mannite—a gummy substance was invariably left, that would not crystallise after exposure to the air for months. Mannitan, or some similar body, may be one of the products.

Dextro-glucose made from honey gave mannite when treated in the same way, having exactly the same melting point as ordinary mannite. In treating milk sugar with dilute sulphuric acid, changing into gallactose and hydrogenising, dulcite was not isolated; but I have not specially studied the reaction.

3. On the Flow of Electricity in Conducting Surfaces. By W. R. Smith, M.A., Assistant to the Professor of Natural Philosophy in the University of Edinburgh. Communicated by Professor Tait. (With a Plate.)

The conditions of a steady flow of electricity in a conducting surface are completely determined, if we know either the nature of the electrical distribution throughout the surface, or the direction and intensity of the flow at every point. On the first of these ways of considering the question, the problem is solved if we can express the potential v at any point as a function of the co-ordinates, and the nature of the distribution will be indicated to the eye by forming the equipotential curves

$$v = \text{const.} \quad . \quad . \quad . \quad . \quad (1).$$

From the second point of view, we should endeavour to determine the lines of flow by equations of the form

$$u = \text{const.} \quad . \quad . \quad . \quad . \quad (2).$$

The curves determined by equations (1) and (2) are obviously orthogonal, and since

$$\frac{d^2v}{dx^2} + \frac{d^2v}{dy^2} = 0,$$

we know, by a theorem of Lamé and Stokes,* that

$$\frac{d^2u}{dx^2} + \frac{d^2u}{dy^2} = 0.$$

Kirchhoff, in the year 1845, took up the problem for plane surfaces† in the first of the two ways we have indicated. By an application of Ohm's law, he expressed analytically the conditions to be satisfied by v. When the electricity enters and issues by a number of individual points, he found (apparently by trial) that an integral of the form $\Sigma(a \log r)$, where r_1 r_2, &c., are the distances of the point (x, y) from the successive points of entrance and issue, satisfies these conditions when the plate is infinite. For a finite plate, it is necessary that the boundary of the plate should be orthogonal to the curves

$$\Sigma(a \log r) = \text{const.} \quad . \quad . \quad . \quad (3).$$

He was thus led to form the orthogonal curves, whose equation he gives in the form

$$\Sigma(a\,[r, \mathrm{R}]) = \text{const.} \quad . \quad . \quad . \quad . \quad (4),$$

where $[r, \mathrm{R}]$ is the angle between r and a fixed line R. These equations he applies to the case of a circular plate, completely determining the curves when there is one exit and one entrance point in the circumference, and showing that in any case a proper number of subsidiary points would make the equipotential lines determined by (3), cut the circumference at right angles. Kirch-

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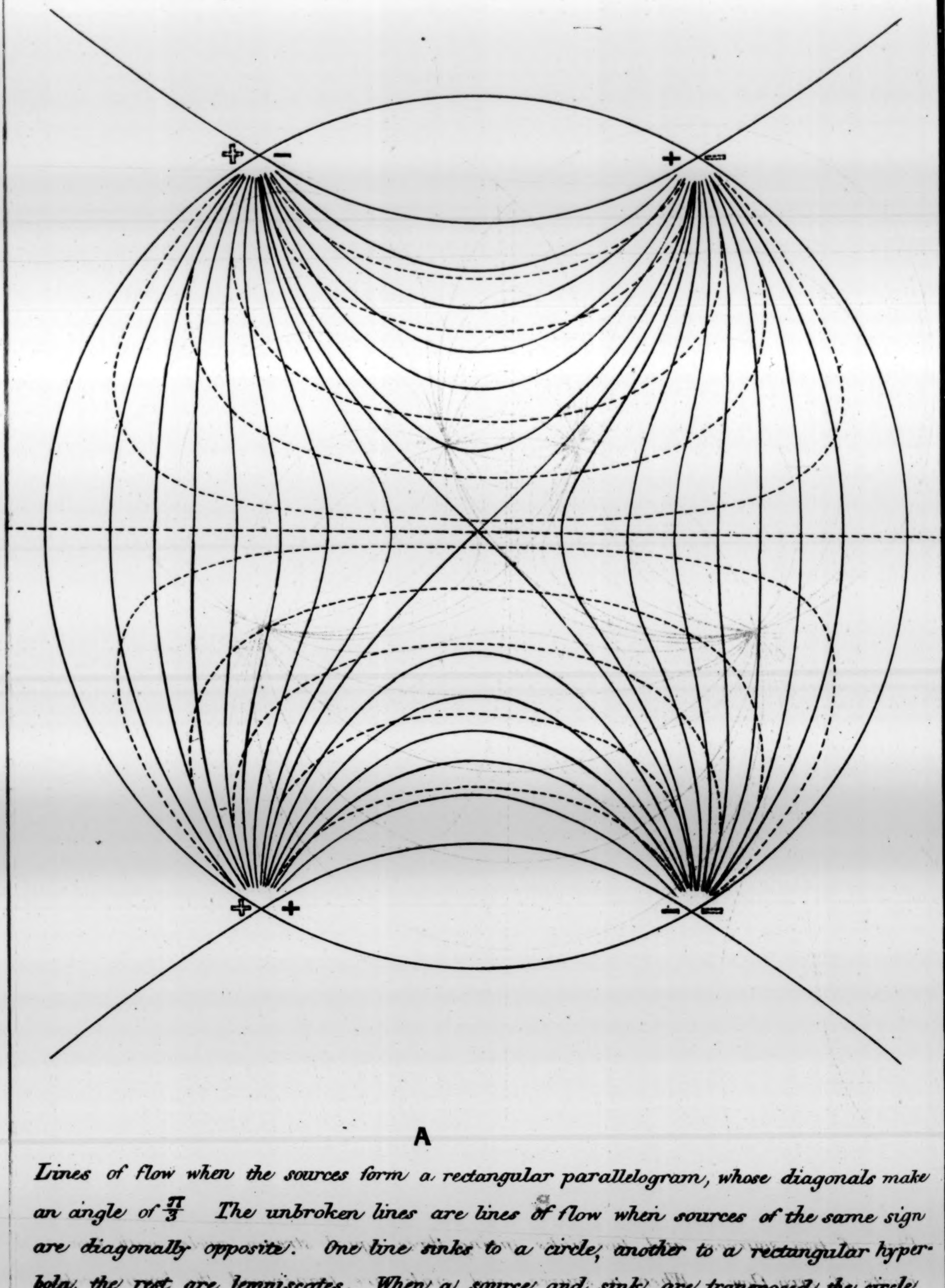
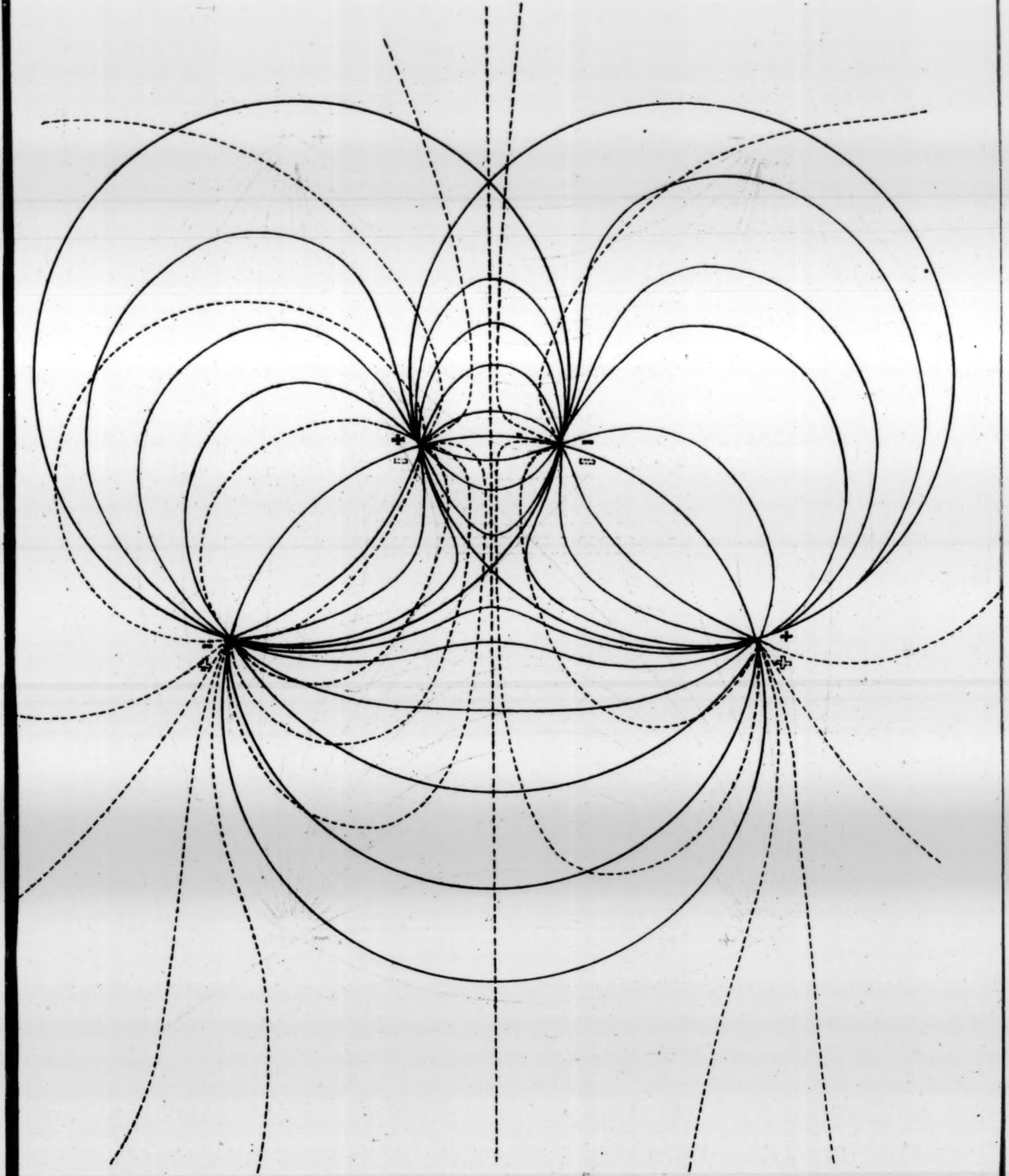
* See Thomson and Tait's Natural Philosophy, i. 542.

† Poggendorff's Annalen, Bd. lxiv.



A

Lines of flow when the sources form a rectangular parallelogram, whose diagonals make an angle of $\frac{\pi}{3}$ The unbroken lines are lines of flow when sources of the same sign are diagonally opposite. One line sinks to a circle, another to a rectangular hyperbola, the rest are lemniscates. When a source and sink are transposed the circle is still part of a stream whose other branch is a straight line, but the lemniscates pass over into the dotted curves.



B

Case of two sources and two sinks giving three equal stream circles with two points of zero flow. When a source and sink are transposed the lower circle is still a line of flow. The other lines assume the dotted form.

hoff's paper is throughout properly busied with the function v, and the stream lines are only dealt with incidentally. There is no attempt to give a physical meaning to the equation (4).

In 1846, Thomson drew attention to the orthogonal systems (3) and (4), as an example of Lamé's theorem.* He showed that the rings and brushes of biaxal crystals are a special case of these curves. They correspond, in fact, as we shall see, to the equipotential lines and lines of flow in an infinite plate with two equal sources of electricity.

Maxwell, in 1856, suggested the application to problems of electric currents of his beautiful theory of the motion of an immaterial incompressible fluid in a resisting medium, but does not appear to have developed the suggestion.†

The object of this paper is to show that, by regarding, in accordance with Maxwell's suggestion, every point of exit or issue as a source or sink, spreading or absorbing electricity, independently of all other sources, Kirchhoff's general equations may be deduced by easy geometrical processes, and extended to certain cases of flow in curved surfaces. We shall, by this method, be naturally led to look mainly at the function u, which in the analytical investigation is subordinated to v. The equation $u = 0$ will receive an obvious physical interpretation, and we shall then proceed to consider in detail the nature of the flow in certain special cases apparently not yet examined.

If a source P, in an infinite uniformly resisting plate, steadily give forth a quantity of electricity E per unit of time, the flow per second over the whole circumference of all circles with P as centre is equal. Hence the rate of flow at each point of the circumference of such a circle is inversely as the radius $= \frac{E}{2\pi r}$. The potential due to P satisfies the equation

$$\frac{dv}{dr} = -\frac{E}{2\pi r},$$

or,

$$v = C - \frac{E}{2\pi}\log r.$$

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* Camb. and Dub. Math. Journ. vol. i. p. 124.

† Cambridge Phil. Trans. vol x.

The potential due to any number of sources P_1, P_2, and sinks P_1' P_2', &c., all of equal power, is got by simple superposition. If E be equal for all points,

$$u = C - \Sigma \frac{E}{2\pi} \log r + \Sigma \frac{E}{2\pi} \log r',$$

where r corresponds to a source, and r' to a sink. Hence the equipotential lines are

$$\frac{r_1\, r_2\, r_3 \ldots}{r_1'\, r_2'\, r_3' \ldots} = C \quad . \quad . \quad . \quad . \quad (5).$$

The equation of the lines of flow follows at once from the equation of continuity. Across any element ds of a stream line subtending angles $d\theta_1$ $d\theta_2$, &c., at the sources, and $d\theta_1'$ $d\theta_2'$, &c. at sinks, no fluid must flow. But the quantity of fluid per second reaching ds from P_n is $\frac{d\theta_n}{2\pi}$ E. The quantity withdrawn by P'_n is $\frac{d\theta'_n}{2\pi}$ E. Hence the differential equation of the stream-line is

$$\Sigma d\theta - \Sigma d\theta' = 0 .$$

Integrating, $$\Sigma\theta - \Sigma\theta' = \text{const.}$$

where θ and θ' are the angles between radii vectores and any fixed lines. If we agree to reckon θ in opposite directions for sources and sinks, the equation becomes

$$\Sigma\theta = a \quad . \quad . \quad . \quad . \quad (6).$$

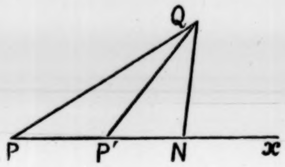
The following are elementary consequences of this equation:—

(*a*.) When we have one source P and one equal sink P′, the stream line through any point Q has for its equation

$$\Sigma\theta = QPP' + QP'P = \frac{\pi}{2} - PQP' = a.$$

Hence the locus of Q is a circle through P and P′, which is Kirchhoff's case. The orthogonals are circles whose centres (R) lie in PP′ produced, and whose radii = $\sqrt{PR.P'R}$.

(*b*.) If we have two equal sources and no sinks, or what is the same thing, sinks at an infinite distance, the stream lines are rectangular hyperbolas. For in this case,

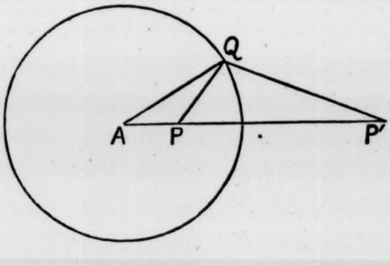


QPN + QP′N = α = QNx, if we make P′QN = QPN. Also QN touches the circle through PP′Q, therefore

$$QN^2 = NP' . NP$$

– the equation of a rectangular hyperbola through P and P′, whose centre is the middle point of PP′, and which is referred to conjugate diameters inclined at angle α. The orthogonal system in this case consists of the lemniscates $rr' = c$. One of the hyperbolas consists of the straight line PP′, and the line equidistant from P and P′. Dividing the plate along the latter line, we have the case of one source in a plate bounded in one direction by an infinite straight line, but otherwise unlimited or bounded by a lemniscate of infinite conductivity, having P and its image due to the boundary line for poles.

(*c.*) To find the image of any point in a circular boundary, *i.e.* to find the source which in combination with a source at the centre of the circle, and an equal sink at any other point, will make the circle a stream line.



Let A be the centre of the circle, and P the given sink. In AP take P′, so that AP.AP′ = AQ². Then PAQ and QAP′ are similar triangles, and QPA = AQP′.

Therefore QAP + QP′A + QPA = 2π, or (6) is satisfied for any point in the circle by assuming at P′ a sink = P.

(*d.*) Hence if there be within a circle m sources and n sinks, we must assume the same number of sources and sinks without the circle, and $n-m$ sources at the centre.

(*e*.) The straight line equidistant from two equal sources of the same sign is clearly a stream line for these points. Hence the image of any point in a straight line is an equal point, which is its optical image.

I have constructed the equation

$$\Sigma\theta = a$$

on the assumption that all the sources are equal, because the degree of the stream line is equal to the number of equal sources (positive and negative) to which the system can be reduced. For if h, k be the co-ordinates of P, the equation becomes

$$\Sigma \pm \tan^{-1}\frac{y-k}{x-h} = \text{C}, \quad . \quad . \quad . \quad (7).$$

If $\left(\frac{y-k}{h-x}\right)_m$ denote the sum of all the combinations of expressions $\pm\frac{y-k}{x-h}$, taken m at a time, we may write this

$$1 - \text{C}\left(\frac{y-k}{x-h}\right)_1 - \left(\frac{y-k}{x-h}\right)_2 + \text{C}\left(\frac{y-k}{x-h}\right)_3 + \left(\frac{y-k}{x-h}\right)_4 - \&\text{c.} = 0 \quad (8),$$

an equation of the n^{th} degree if there be in all n sources.

The degree of the equipotential lines is also $= n$ if there be an equal number of sources and sinks. In general, if there be m sources of one sign, and $n-m$ of another, and $m > n-m$, $2m$ is the degree of the equipotential lines. This is one of many features which make it more convenient to work with stream lines.

It is obvious from equation (8), that every stream line must pass through all the sources. Thus, the circle in case (*c*), which passes through no source, is not a complete stream line, the other branch being the straight line APP′, which passes through all the sources. Distinct stream lines can intersect only at a source, for at no other point can $\Sigma\theta$ be indeterminate. Where two branches of the same stream line intersect the velocity is necessarily zero, changing sign in passing through the point. The physical meaning of a branch is that two streams impinge, and are thrown off with an abrupt change of direction.

The same result is easily found from the analytical condition for a singular point $\frac{du}{dx} = \frac{du}{dy} = 0$.

$$\text{For } -\frac{du}{dx} = \frac{dv}{dy} = \text{velocity parallel to axis of } y,$$

$$\frac{du}{dy} = \frac{dv}{dx} = \text{velocity parallel to axis of } x,$$

or directly by differentiation.

$$\left.\begin{aligned} \frac{du}{dx} &= \Sigma\left(\mp\frac{y-k}{r^2}\right) \\ \frac{du}{dy} &= \Sigma\left(\pm\frac{x-h}{r^2}\right) \end{aligned}\right\} \quad . \quad . \quad . \quad (9).$$

The nature of the intersection of the branches of a stream line at a multiple point is easily determined.

At an m-point, the angles at which the branches cut the axis of x are the roots of the equation—

$$\left(\frac{d}{dx} + \tan\phi\,\frac{d}{dy}\right)^m u = 0 \quad . \quad . \quad . \quad (10).$$

Where, since $\quad \frac{d^2u}{dx^2} = -\frac{d^2u}{dy^2}$

$$\frac{d^mu}{dx^m} = -\frac{d^mu}{dx^{m-2}dy^2} = \frac{d^mu}{dx^{m-4}dy^4} \text{ \&c.},$$

$$\frac{d^mu}{dx^{m-1}dy} = -\frac{d^mu}{dx^{m-3}dy^3} \text{ \&c.}$$

Whence (10) becomes

$$\frac{d^mu}{dx^m}\left(1 - \frac{m\,.\,m-1}{1\,.\,2}\tan^2\phi + \text{\&c.}\right) +$$

$$\frac{d^mu}{dx^{m-1}dy}\left(m\tan\phi - \frac{m\,.\,m-1\,.\,m-2}{1\,.\,2\,.\,3}\tan^3\phi + \text{\&c.}\right) = 0.$$

We can choose the axes so that $\frac{d^mu}{dx^m} = 0$, and reduce the equation to

$$m\tan\phi - \frac{m\,.\,m-1\,.\,m-2}{1\,.\,2\,.\,3}\tan^3\phi + \ldots = 0 \quad . \quad (11),$$

or
$$\tan m\phi = 0 \quad . \quad . \quad . \quad (12),$$

$$\phi = \frac{l\pi}{m}, \text{ where } l \text{ is any integer from 1 to } m.$$

Thus the branches make equal angles with each other. This proposition depends solely on the relation $\nabla u = 0$. It is therefore true, also, for the equipotential lines, as is otherwise obvious.*

The general nature of the stream lines will be different, according as the number of sinks is or is not equal to the number of sources. In the former case, $\Sigma(\theta) = 0$ is satisfied at all points infinitely distant, the radii being all parallel, and the positive and negative angles equal in number. Hence one stream line has the straight line at infinity as a branch, or intersects the straight line at infinity at right angles, and therefore has an asymptote. This stream line will, in general, be of the $n-1^{\text{th}}$ degree. In some cases it may be of a lower degree; as, for example, when the conic at infinity is its other branch. A case of this sort will be given below. The other stream lines of the system cannot meet the line at infinity, and cannot have asymptotes. However far they run out, they must therefore loop and return.

When there are more sources than sinks, $\Sigma\theta$ becomes indeterminate at an infinite distance, as might have been anticipated from the fact, that in this case there is a constant flow of electricity outwards, implying a sink at an infinite distance. The line at infinity is not in this case a stream line, and will be cut by all the stream lines, which do not loop except at finite distances, and have all asymptotes.

The asymptotes, in this case, may be easily constructed by the aid of equations (6) and (8).

At the infinitely distant point of contact the velocities due to all sources are in the same direction, or the asymptote must be parallel to the radii.

If there are m sources and $n-m$ sinks, the stream line whose asymptote makes an angle α with the initial line is obviously

$$\Sigma\theta = (2m-n)\alpha = \tan^{-1} C \quad . \quad . \quad . \quad (13).$$

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* I have since found that this result has been already proved for plane curves by Professor Rankine and Professor Stokes (Proc. R.S., 1867), and for spherical harmonics by Sir W. Thomson and Professor Tait, in their treatise on Natural Philosophy.

This equation has $2m-n$ roots.

$$\alpha_1,\ \alpha_1+\frac{\pi}{2m-n},\ \alpha_1+\frac{2\pi}{2m-n},\ \&c.$$

So that each stream line has $2m-n$ asymptotes equally inclined to one another.

Transforming to rectangular co-ordinates, and choosing the asymptote as axis of x, (8) reduces to

$$\left(\frac{y-k}{x-h}\right)_1-\left(\frac{y-k}{x-h}\right)_3+\ \ .\ \ .\ \ =0.$$

When $y=0$, x has two roots $=\infty$ if

$$\Sigma(\pm k)=0\quad .\quad .\quad .\quad .\quad (14).$$

Hence the asymptote is such that the algebraic sum of the perpendiculars from the sources diminished by the sum of the perpendiculars from the sinks is zero. It is obvious without analysis that this condition is necessary, that the velocity perpendicular to the asymptote, at its point of contact with the curve, may be absolute zero. If sinks weigh upward, all lines passing through the centre of gravity of the system are asymptotes, and $2m-n$ of these lines, equally inclined to each other, belong to one stream line. The system must have a centre of gravity, for by pairing sources and sinks we produce couples which will always give a single resultant when compounded with the weights of the extra sources.

A complete system has no centre of gravity, but (14) is satisfied for all lines perpendicular to the axis of the resultant couple. If the axis of the couple formed by pairing a source and sink at distance ρ_m makes an angle ψ_m with the axis of the resultant couple

$$\Sigma(\rho\sin\psi)=0\quad .\quad .\quad .\quad (15),$$

an equation with only one root to determine the direction of the asymptote. In this case the asymptote meets the curve in a double point, and has contact of the third order, or x has three roots $=\infty$.

The condition for this is obviously—

$$\Sigma(\pm kh)=0\quad .\quad .\quad .\quad (16),$$

which since $\Sigma(\pm k)=0$, does not depend on the point of the asymptote from which h is reckoned.

If (15) is satisfied identically, the asymptote meets the curve in a triple point. Two of the branches belong to the line at infinity, and the finite branch sinks to the $n-2$ degree.

In this case not only $\Sigma(\pm k)=0$, but $\Sigma(\pm h)=0$. Hence (16) no longer gives a fixed point on the asymptote, but only fixes its direction. A further analytical condition is easily found, but is unnecessary. For in this case the centre of gravity of the sources coincides with the centre of gravity of the sinks. The stream lines due to the sources alone would have the same sets of asymptotes as those due to sinks. One of these sets is necessarily asymptotic in the complete system, which has always one line with real asymptotes. The set will consist of $\frac{n}{2}$ rays, all passing through the common centre of gravity of the sources and sinks, and equally inclined to one another.

Rectilineal Branches are asymptotes coinciding with their curves.

Hence, in an incomplete system, all straight lines pass through the centre of gravity of the system, and belong to one stream line, unless the centre of gravity be a source. In any case they are equally inclined to one another, for if not branches of one stream line, they would be so for the system got by removing the source at their intersection.

In a complete system there can be only one rectilineal stream line, unless sinks and sources have a common centre of gravity. In the latter case, there can be at most $\frac{n}{2}$ straight lines, forming equally inclined rays through that point.

The condition for a rectilineal branch is in general that the sources must be either on the line or be two by two, each other's images on the line. For if not, remove all the sources on the line and all pairs of sources which are each other's images in the line. Next, remove all sources on one side of the line by placing equal sources of opposite sign at the place of their images The straight line is still a stream line, and on one side of it there are no sources, and therefore constant potential, which is absurd. Similarly it can be shown that a circle is a possible stream line only when the sources are on the curve or image each other. From this it follows that no finite number of sources can give parallel rectilineal streams or non-intersecting circular streams.

A similar investigation applies to equipotential lines. The image of a point in a rectilineal equipotential line is the same in position as the image in a stream line, but of opposite sign. No source can lie on an equipotential line. Hence, to show that for right equipotential line the points must image two by two, we have only to remove all sources on one side of the line, placing equal sources of the same sign at their images. The line is still equipotential, therefore we may suppose it charged to constant potential, and all sources removed. Hence all stream lines become rectilineal, which is absurd. Similarly if a circle is equipotential, the sources must balance about it two by two, *i.e.*, must be in a straight line with its centre, at distances to which the radius is mean proportional—otherwise we can find a system reducible to a single point at the centre of the circle, and in which all stream lines are rectilineal. Hence, no incomplete system can have a rectilineal or circular potential line.

Points of Inflexion occur at all points on the locus—

$$\frac{d^2u}{dx^2} + 2\,\frac{d^2u}{dxdy}\,\frac{dy}{dx} + \frac{d^2u}{dy^2}\cdot\left.\overline{\frac{dy}{dx}}\right|^2 = 0 \quad . \qquad (17).$$

Remembering that

$$\frac{d^2u}{dx^2} = -\frac{d^2u}{dy^2} = \Sigma\left(\pm\frac{\sin 2\theta}{r^2}\right),$$

$$\frac{d^2u}{dxdy} = \Sigma\left(\mp\frac{\cos 2\theta}{r^2}\right),$$

we can readily bring (17) into the form—

$$\Sigma\left(\frac{\sin 2\theta}{r^2}\right)\Sigma\left(\frac{\cos(\theta+\theta')}{rr'}\right) - \Sigma\left(\frac{\cos 2\theta}{r^2}\right)\Sigma\left(\frac{\sin(\theta+\theta')}{rr'}\right) = 0,$$

or,

$$\Sigma\,\frac{\sin(2\theta-\theta'-\theta'')}{r^2r'r''} = 0 \quad . \quad . \quad . \quad (18).$$

In this last expression θ' and θ'' may assume the value θ.

The radius of curvature may be similarly expressed, but such expressions can hardly have a practical application.

The cases of practical interest are mainly those where the number of sources is small. We have already examined the cases of two sources of the same or opposite signs. We will now proceed to consider the cases that arise when there are three or four sources.

Three Sources.—In general the curves will be cubic passing through the three sources, and having asymptotes determined as above. The direction of flow at any point of the field may be found by observing that if φ be the angle between the tangent and a radius vector,

$$\Sigma \pm \frac{\sin \varphi}{r} = 0 .$$

It will sometimes be possible to find the direction of flow geometrically by the following obvious theorem.

If a circle be described touching a stream line at any point, and cutting off from the radii vectores of that point, fractions of their lengths, μ μ', &c., where μ is negative if the point of intersection is in the radius vector produced, and also negative if the radius vector is drawn from a sink, then—

$$\Sigma (\mu) = 0 .$$

When the number of sources is large this theorem is not in general convenient, but it is often applicable where there are only three points.

The lines of flow can, however, be readily described with any degree of accuracy when there is one sink, by describing segments of circles with constant difference of angle through the sink and one source, and drawing through the other source straight lines with the same difference of angle. The stream lines will be diagonals of the quadrilaterals into which the field is thus divided. The process may be extended to the case of two sources and two sinks by taking the intersections of two sets of circles.

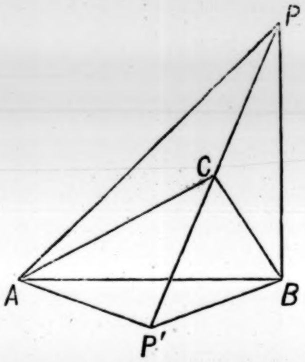
When there are two sources and one sink, the singular points may be found by an easy geometrical method. Let A, B, be sources, C the sink, and P a point of zero velocity. The resultant velocity due to A and C is in the tangent to the circle PAC, and also—since P is a singular point—in the line PB. Therefore—

$$\mathrm{BPC} = \mathrm{PAC} .$$

Similarly

$$\mathrm{APC} = \mathrm{PBC} .$$

Hence PCA, BCP are similar triangles, and there are two points of zero flow, P and P′, lying in the line bisecting the angle C, and such that PC is a mean proportional to BC and AC. The directions of the orthogonal branches at P bisect the angle APB and its supplement.



For the initial line is a tangent at the singular points if

$$\frac{d^2u}{dx^2} = \Sigma\left(\pm\frac{\sin 2\theta}{r^2}\right) = 0 \quad . \quad . \quad (19).$$

Let now APC $= \alpha$, BPC $= \frac{C}{2} - \alpha = \beta$, and assume the bisector of APB as initial line. Then

$$\sin\frac{C}{2}\left(\frac{1}{PA^2} - \frac{1}{PB^2}\right) + \frac{\sin\overline{\alpha-\beta}}{PC^2} = \Sigma\frac{\sin 2\theta}{r^2},$$

which since

$$\left.\begin{aligned} \frac{1}{PA} &= \frac{1}{PC^2}\cdot\frac{\sin^2\beta}{\sin^2\frac{C}{2}} \\ \frac{1}{PB^2} &= \frac{1}{PC^2}\cdot\frac{\sin^2\alpha}{\sin^2\frac{C}{2}} \end{aligned}\right\}$$

becomes,

$$\sin^2\beta - \sin^2\alpha - \sin\overline{\alpha-\beta}\,.\,\sin\overline{\alpha+\beta} = 0,$$

which satisfies (20).

The chief interest lies in the cases where the cubic breaks up into a straight line and a conic. This takes place for one stream line of the system when all the sources lie on a straight line, or when they form an isosceles triangle with points of the same sign at the base. The cases are—

1. *Two Sources and a Sink.*—The conic is always a *circle* with the sink as centre. If the sink lies in the line of the sources produced, the radius of the circle is a mean proportional to the distances of the sink from the sources. If the sink lie between the sources, the circle is impossible. If the sink is the vertex of an isosceles triangle, the circle passes through both sources, and all asymptotes meet in the point of zero flow furthest from the sources. If the sink is half way between the sources, there are two straight lines and a real and impossible circle.

2. *Three Sources of the same Sign.*—Every stream line has three asymptotes, meeting in the centre of gravity, and inclined at angles of $\frac{\pi}{3}$. If one of these asymptotes becomes a branch, the other branch is a hyperbola, with centre of gravity as centre, and axes in ratio of $\sqrt{3}$ to 1. If the points form an isosceles triangle, the hyperbola passes through the extremities of the base. If the triangle is equilateral, the hyperbola coincides with its asymptotes. If the vertical angle is less than $\frac{\pi}{3}$, the rectilineal branch is the transverse axis; if greater than $\frac{\pi}{3}$, it is the conjugate. If the points are all in a line, the vertices of the hyperbola lie on that line, and are the points of zero flow, which are easily found. If one point is half way between the other two, we have two rectilineal branches and two hyperbolas, the conjugate axis of the one being equal to the transverse axis of the other. The hyperbolas are, therefore, confocal.

Four Points.—Complete System.

Singular Points.—If A and B are sources, C and D sinks, there is a singular point at P, if the circles APC, BPD, and also APD, BPC touch at P. Hence, there are no real singular points if the sides of the quadrilateral ACBD intersect, unless all the points be on a circle, which in this case contains all the singular points.

Straight Lines.—The one stream line which has an asymptote is of the third degree. If a straight line is one factor, the other factor is a conic, which is always a circle. For if A, C are the images of B, D respectively in the straight line, a circle can be drawn through them, which is obviously the branch sought. But if A, B lie without the line, and C, D on it, a circle through A, B having its centre O in CD produced, so that OA is a mean proportional between OC and OD is the circle required. If ABCD are all on a straight line, the other branch is manifestly a circle with centre on the line.

Conics.—The parabola is an impossible conic for any finite number of points. For the parabola has two asymptotes meeting at infinity. Hence the centre of gravity of an incomplete system, or of the sinks and sources separately in a complete system, must be at an infinite distance, which is absurd. The conics are therefore central.

The *hyperbola*, which has two asymptotes, is only possible when the cubic reduces to a conic. This demands that the centre of gravity of sinks and sources shall coincide, *i.e.*, that AB, CD are diagonals of a parallelogram. The asymptotes must meet at right angles, and the hyperbola is equilateral. It is obvious, indeed, that in this case the sources and sinks give separately sets of concentric rectangular hyperbolas, of which the one passing through the four points belongs to both sets, and is the only asymptotic curve of the complete system.

In this case the equipotential lines are lemniscates. Let the origin be the centre of the system, $2a$ and $2b$ the diagonals of the parallelogram, α and β their angles with the initial line. At any point P

$$\mathrm{AP}^2 \,.\, \mathrm{BP}^2 + \lambda \mathrm{CP}^2 \,.\, \mathrm{DP}^2 = 0 \,.$$

That is,

$$r^4 + a^4 - 2a^2r^2 \cos 2\ \overline{\theta - \alpha} + \lambda(r^4 + b^4 - 2b^2r^2 \cos 2\ \overline{\theta - \beta}) = 0$$

$$(1 + \lambda)(r^4 + a^4) - 2r^2 \cos 2\theta\ (a^2 \cos 2\alpha + \lambda b^2 \cos 2\beta)$$
$$+ 2r^2 \sin 2\theta\ (a^2 \sin 2\alpha + \lambda b^2 \sin 2\beta) = 0 \,.$$

When $\lambda = -\dfrac{a^2 \sin 2\alpha}{b^2 \sin 2\beta}$, the curve becomes

$$(b^2 \sin 2\beta - a^2 \sin 2\alpha)(r^4 + a^4) - 2a^2b^2r^2 \sin 2(\beta - \alpha) \cos 2\theta = 0 \,,$$

a lemniscate, with foci on the initial line, and centre at the origin.

If the parallelogram is a rectangle $a = b$, and the curve is

$$r^4 - 2a^2r^2 \frac{\cos \overline{\beta - \alpha}}{\cos \overline{\beta + \alpha}} \cos 2\theta + a^4 = 0 .$$

It is easily shown that the stream lines orthogonal to these are lemniscates with the same centre, passing through the four points, one of which becomes a circle when the parallelogram is rectangular.

The ellipse appears to be an impossible conic for four points, for conics occur in pairs orthogonal to each other. The orthogonal of the ellipse must be a confocal hyperbola, which is impossible, the only hyperbola being that discussed above. Orthogonal circles, however, are possible, and fall under two classes, according as all the points are on one circle, or two on each.

If ABCD lie on a circle, that circle is obviously a stream line. Let BA.DC produced meet in O. Then OA.OB = OC.OD, and the circle, with centre O and radius $\sqrt{OA.OB}$ is the other branch of the stream line. If O lies within the circle ABCD, the second circle becomes impossible. If CA.BD produced meet R and CB.AD in S, R and S are centres of equipotential circles, only one of which is real, unless the second stream circle is imaginary. We may take as an example the case of a rectangle, points of the same sign lying on the same diagonal. Let the circle through the four points be ($2a$ and $2b$ being the sides of the rectangle)

$$x^2 + y^2 - a^2 - b^2 = 0 .$$

The other branch is the imaginary circle

$$x^2 + y^2 + a^2 + b^2 = 0 ;$$

and we know that another stream line is the hyperbola

$$y^2 - x^2 - a^2 + b^2 = 0 .$$

Hence the stream lines are

$$(x^2 + y^2)^2 - (a^2 + b^2)^2 + \lambda(y^2 - x^2 - a^2 + b^2) = 0 ,$$

lemniscates as above.

The equipotential circles degenerate into the straight lines

$$x = 0 \quad \text{and} \quad y = 0 .$$

If O be the point in CD produced which is equidistant from A and B, and OC.OD = OA^2 = OB^2, the circle with O as centre passing through A,B is a line of flow.

The circle having its centre P in AB produced, and passing through CD, is obviously orthogonal; and since PA.PB = PC^2

$= \mathrm{PD}^2$ is also a line of flow. In this case both circles are necessarily real.

It is clearly impossible that the same system should have two pairs of circular stream lines of either of the classes we have analysed. Nor can two complete pairs of different classes occur, since otherwise two stream lines would intersect. But three real and an imaginary circle are possible, if ABCD lie on a circle, and at the same time obey the condition for a pair of circles of the second class, that is, if AB produced pass through the pole of CD with respect to the circle ABCD. The three circles are manifestly orthogonal, and their radical centre is centre of the fourth (imaginary) circle.

If the circle through ABCD is

$$\mathrm{S} = x^2 + y^2 - a^2 = 0\,,$$

the lines AB, CD respectively

$$u = hx + ky - a^2 = 0$$
$$v = h'x + k'y - a^2 = 0\,,$$

we have

$$hh' + kk' - a^2 = 0\,,$$

and the second and third circles become

$$\mathrm{S} - 2u = 0$$
$$\mathrm{S} - 2v = 0\,.$$

The fourth or imaginary circle is

$$\mathrm{S} - 2w = 0\,,$$

where

$$w = \frac{a^2(k'-k)x}{hk' - kh'} + \frac{a^2(h-h')y}{hk'-kh'} - a^2\,,$$

$w = 0$ representing the polar of the intersection of AB, CD.

Thus the equation to the stream lines may be written

$$(\mathrm{S} - 2u)(\mathrm{S} - 2v) + \lambda\mathrm{S}(\mathrm{S} - 2w) = 0\,,$$

or,

$$(1 + \lambda)\mathrm{S}^2 - 2(u + v + \lambda w)\mathrm{S} + 4uv = 0\,,$$

which degenerates into a cubic when $\lambda = -1$.

The equations may, in general, be simplified by a proper choice of co-ordinates.

Take, for example, the case when $S-2u$, $S-2v$ are equal circles.

Then
$$h^2 + k^2 = h'^2 + k'^2,$$

and by proper choice of axes,

$$h = -h'$$
$$k = k'$$
$$k^2 - h^2 = a^2.$$

Hence,
$$w = \frac{a^2y}{k} - a^2.$$

The lines become

$$(1+\lambda)S^2 - 2\left(2ky - 2a^2 + \frac{\lambda a^2 y}{k} - \lambda a^2\right)S + 4(ky-a^2)^2 - 4h^2x^2 = 0.$$

If the three circles are equal, we have further,

$$h^2 + k^2 = 2a^2$$
$$h = \frac{a}{\sqrt{2}}$$
$$k = \frac{\sqrt{3}a}{\sqrt{2}}$$

Accurate drawings of this case, and of the lemniscates in the case of a rectangular parallelogram, have been prepared, to accompany this paper, by Messrs Meik and Brebner, in the Physical Laboratory of the University. The dotted lines in these diagrams show the lines of flow when the signs of a source and sink are transposed.*

Verifications have been sought by determining equipotential lines experimentally, and superposing them upon drawings of the stream lines. The experiments were executed by students in the Physical Laboratory. The process employed was essentially that of Kirchhoff, but the use of Thomson's galvanometers has made it much more rapid, as well as more delicate.

Spherical Surfaces.—To extend the method above used to spherical surfaces, we must take as starting point, not a single source, but a source and sink at the extremities of a diameter. For brevity, we shall speak only of the source, assuming the existence of a corresponding sink.

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* That a greater variety of curves might be given, without overcrowding the figure, the two sides of one of the diagrams have been made unsymmetrical, some of the curves being given (in half) on the one side, others on the other.

When there is one source, the stream lines are manifestly great circles through it, and the equipotential lines small circles, of which it is the pole.

If the radius of the sphere is a, the circumference of the small circle, whose angular radius is θ, is $2\pi a \sin\theta$. Hence if u be the potential,

$$\frac{du}{d\theta} \propto \frac{1}{\sin\theta}$$

$$u \propto \frac{1}{2}\log\frac{1-\cos\theta}{1+\cos\theta}$$

For any number of sources the potential will be

$$\frac{1}{2}\left(\Sigma \pm \log\frac{1-\cos\theta}{1+\cos\theta}\right),$$

and the equation of the equipotential lines,

$$\frac{1-\cos\theta_1}{1+\cos\theta_1}\cdot\frac{1-\cos\theta_2}{1+\cos\theta_2}\cdots = C\,\frac{1-\cos\theta'_1}{1+\cos\theta'_1}\cdot\frac{1-\cos\theta'_2}{1+\cos\theta'_2}\cdots,$$

the accented angles belonging to sinks.

For the lines of flow we have, precisely as in a plane, $\Sigma(\pm\varphi)=c$, where φ is the angle between the great circle through a source and a point on the line, and a fixed great circle through the source.

Let us take, as an example, the case of one source and one sink. Let the co-ordinates of these points be $h, k, 0$; $h, -k, 0$, and those of any point on an equipotential line, x, y, z.

We have for the equation of this line,

$$\frac{1-\cos\theta}{1+\cos\theta} + \lambda\,\frac{1-\cos\theta'}{1+\cos\theta'} = 0,$$

where

$$\cos\theta = \frac{hx+ky}{a^2},\ \cos\theta' = \frac{hx-ky}{a^2}.$$

Hence the projections of the equipotential lines on the plane of xy have as equation,

$$(a^2-hx-ky)(a^2+hx-ky)+\lambda(a^2-hx+ky)(a^2+hx+ky)=0,$$

or—

$$a^4 + k^2y^2 - h^2x^2 - 2\,\frac{1-\lambda}{1+\lambda}\,a^2ky = 0$$

—a series of similar hyperbolas, whose centres lie on the axis of y, whose axes are parallel to the co-ordinate axes, and inversely proportional to the co-ordinates of the source, and which all cut the axis of x at points distant $\pm\,\frac{a^2}{h}$ from the origin. Obviously one of the lines is the great circle perpendicular to the line joining the sources.

For the stream lines we have in this case,

$$\varphi - \varphi' = c,$$

observing that

$$\tan\varphi = \frac{az}{xk - hy}$$

$$\tan\varphi' = \frac{-az}{xk + hy}\,.$$

This equation becomes

$$k^2x^2 - h^2y^2 - a^2z^2 + \lambda xz = 0\,,$$

a cone which intersects the tangent plane to the sphere at the extremity of the axis of x, in a series of similar ellipses, having their centres on the intersection of the plane with the plane of xz, and passing through the points $a, \pm\,\frac{ak}{h}, 0$. Two of the stream lines are manifestly great circles, whose equations are $x = 0$ and $z = 0$.

If we divide the sphere along the former of these circles, we cut off the subsidiary source and sink, and get the case of a hemisphere, in which the source and sink are equidistant from the pole. A curious hemispherical case is got by dividing the sphere along the equipotential hemisphere. In this case we have two sources of the same sign within the hemisphere, one being the subsidiary source of the removed sink. But in order that the distribution may remain unchanged, we must have the potential maintained constant at the edge of the hemisphere. This may be effected by making the base a conductor with a sink at its centre, or, indeed, by placing the sink at the vertex of any conducting surface of revolution which joins the hemisphere. From these hemisphere cases, obvious cases of half and quarter hemispheres follow.

4. On the Kombi Arrow-Poison (*Strophanthus hispidus*, DC.) of the Manganja district of Africa. By Dr Thomas R. Fraser.

(*Abstract.*)

In nearly every narrative of exploration in uncivilised tropical regions, accounts are given, often no doubt somewhat fanciful, of poisonous substances which are said to possess the most remarkable properties. Usually these poisons are of vegetable origin; and the great majority may be included in the two divisions of *ordeal* and of *arrow* poisons, according as they are applied to one or other of these purposes. Among the most remarkable of the *ordeal-poisons* are the *Tanghinia venifera* of Madagascar, the *Physostigma venenosum* of Old Calabar, and the Akazga poison of the Gaboon; and of the *arrow-poisons*, the famous Curara or Wourali of South America, and the *Antiaris toxicaria* of Java.

The examination of these substances has not only proved of great value to physiology, but practical medicine has likewise been benefited—one of them, at least, being now an important medicinal agent.

In bringing before the Society a few of the results of a recent examination of a new arrow-poison, the author has to express his gratitude to the President, who very kindly gave him the specimens of poison with which the experiments have been made. These specimens, consisting of a number of ripe follicles, were sent to Dr Christison by Mr Walker, and were collected in the expedition of the late Bishop M'Kenzie.

Several specimens of the poison have likewise been sent to Professor Sharpey by Dr Kirk, H.M. consul at Zanzibar. Dr Kirk says "that the plant is a woody climber, growing in the forest, both of the valley and hills, and found at various places between the coast and the centre of the continent, above the Victoria Falls of the Zambesi. The stem is several inches in diameter, and rough outside. The plant climbs up the highest trees, and hangs from one to the other like a bush vine. The flowers are of a pale yellow, and last for but a short time during the months preceding the first rains of the season (October and November). The fruit is ripe in June, and collected by the natives, who separate the rough outer coat before drying it, preserving the more leathery inner covering and the seeds."*

Dr Livingstone gives some interesting information regarding the poison in his "Narrative of an Expedition to the Zambesi and its Tributaries." He mentions that arrows poisoned with it are used for killing wild animals only; arrows destined for the more noble object of killing men being poisoned with the entrails of a small caterpillar. Dr Livingstone says that in hunting, the natives follow the game with great perseverance and cunning:—"The arrow, making no noise, the herd is followed until the poison takes effect, and the wounded animal falls out; it is then patiently watched till it drops; a portion of meat round the wound is cut away, and all the rest eaten" (p. 465).

Dr Livingstone also says that the poisoned arrows are made in two pieces. "An iron barb is firmly fastened to one end of a small wand of wood, ten inches or a foot long, the other end of which, fined down to a long point, is nicely fitted, though not otherwise secured, in the hollow of the reed which forms the arrow-shaft. The wood immediately below the iron head is smeared with the poison. When the arrow is shot into an animal, the reed either falls to the ground at once, or is very soon brushed off by the bushes; but the iron barb and poisoned upper part of the wood remain in the wound. If made in one piece, the arrow would often be torn out, head and all, by the long shaft catching in the underwood, and striking against trees" (p. 466).

The follicles examined by the author vary in *length* from about nine and three-fourths, to about twelve and one-fourth inches, and in greatest *thickness* from about one inch to three-fourths of an inch, and they vary in *weight* from about 130 to 330 grains. They contain from 100 to 200 seeds, each of which weighs about half a grain, and has attached to it a beautiful comose appendix, placed on an extremely brittle stalk. For the identification of the plant the author is indebted to Professor Oliver of Kew, who writes, in a letter dated 10th Dec. 1869,—"I reopen your note to say that I have just dissected a flower, and conclude to name the Kombi plant *Strophanthus hispidus*, DC." This plant belongs to the natural order *Apocynaceæ*.

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* Extract from letter to Professor Sharpey, dated January 1, 1864.

When the seeds contained in these follicles are bruised and treated in a percolator with rectified spirit, a greenish yellow tincture is obtained. By distilling off the greater part of the spirit, and drying the residue on a water bath, and in the exhausted receiver of an air-pump, an extract is procured which weighs about 25 per cent. of the seeds employed, has an intensely bitter taste, and contains about one half of its weight of an inert fixed oil. From this extract the author has succeeded in separating a very powerful active principle.

As, however, the greater number of the experiments have been made with the extract, the results of these experiments only will be described in the following brief account of the physiological action of the Kombi arrow-poison, it being understood that the action of the active principle is of the same character.

When a small dose ($\frac{1}{20}$th of a grain) of this extract is mixed with a few minims of water, and injected under the skin of a frog, no distinct symptom is seen until about half an hour, when the animal's movements become somewhat sluggish. Soon afterwards the respirations cease, some stiffness occurs in the thoracic extremities, reflex sensibility diminishes, some stiffness appears in the pelvic extremities, and in about two hours after the administration, voluntary movements entirely cease, and strong galvanic irritation produces no effect, even when applied to exposed muscles and nerves. An examination of the heart shows that it is completely paralysed, the ventricles being pale and contracted, while the auricles are dark and distended.

It was obviously suggested by these phenomena that this substance acts as a cardiac poison; and, accordingly, some experiments were made in which the heart was exposed before the administration, of which the following is an example:—

One-tenth of a grain of extract was injected under the skin of a frog. Five minutes thereafter, it was observed that the ventricular systole was somewhat prolonged; in six minutes, the ventricular diastole was imperfect, so that only portions of the ventricle dilated to admit blood from the auricles; in six minutes and thirty seconds, the greater portion of the ventricle was continuously pale and contracted, each auricular systole propelling merely a small drop of blood into the ventricle, where it produced a dark, pouch-like projection, which at times disappeared, and at other times only changed its position during the imperfect systole of the ventricle; in seven minutes, the ventricle altogether ceased to contract, while the movements of the auricles continued at nearly the normal rate; and in eighteen minutes, the auricles in their turn became motionless, but, in place of being contracted and empty like the ventricle, they were distended and full of dark blood. Notwithstanding this absolute paralysis of the heart, respiratory movements occurred for thirty-five minutes after the ventricle had ceased to contract, and the frog jumped about actively for some time after this.

The experiments that have been performed with birds and mammals have likewise shown that this poison acts primarily on the heart.

An endeavour was made to ascertain by what mode of action these very peculiar cardiac effects are produced. With this object experiments were made, in which the cerebro-spinal axis was completely destroyed, in which the vagi nerves were divided, and in which the peripheral terminations of the vagi were paralysed by atropia, previously to the exhibition of the Kombi-poison; but no important modifications were thereby caused, and it is therefore obvious that the action on the heart is not exerted through the cerebro-spinal nerves. In other experiments, after complete cardiac paralysis, the surface of the heart was irritated by galvanic and other stimulants, but no effect was thereby caused.

Another very prominent action of this poison is that exerted on the voluntary muscles, by which their activity is gradually impaired, and finally completely destroyed, so that the muscles are quickly in a condition of true *rigor mortis.*

Regarding the other physiological effects, it is sufficient briefly to mention that the sensory and motor spinal nerves, the abdominal and cervical sympathetics, and the muscular walls of the stomach, intestines, bladder, and uterus, are paralysed at an early stage, although not until the blood-heart has ceased to contract; while the lymph-hearts of the frog maintain a normal rate long after paralysis of the blood-heart.*

From these results it is apparent, that the primary action of the Kombi arrow-poison is isolated in the heart, and that it may therefore be included in the class of the *cardiac poisons,*—a class of poisons whose action has been most accurately defined by the researches of Kolliker, Vulpian, Pélikan, Hammond and Weir Mitchell, Hilton Fagge and Stevenson, Holme, Dibkowsky, and others.

5. On Thebo-lactic Acid. By J. Y. Buchanan, M.A.

Thebo-lactic acid was discovered in Turkey opium by Messrs T. & H. Smith, the eminent morphia manufacturers of this city. It was examined by Stenhouse, and found to have the same composition as lactic acid, from which, however, it was supposed by the Messrs Smith to differ in the crystalline form of its copper and morphia salts. At present we are acquainted with three isomeric lactic acids, two of them differing from each other chemically, whilst the third is distinguished by its power of rotating the plane of polarisation of light. The last named acid, having been but recently† discovered, it is impossible to say whether it possesses any decidedly distinctive chemical properties or not. The other two, namely, the ordinary or ethyliden—and the ethylen-lactic acids, possess perfectly distinct chemical properties, determined by the different relative position in each of the alcoholic hydroxyl. The following rational formulæ express the different constitution of the two acids:—

$$\begin{array}{c} CH_3 \\ CHOH \\ COOH \end{array}$$

Ordinary lactic acid.

$$\begin{array}{c} CH_2OH \\ CH_2 \\ COOH \end{array}$$

Ethylen-lactic acid.

They may be distinguished at once by replacing in each the alcoholic hydroxyl by chlorine. We thus obtain from ordinary lactic acid the so-called α-, from ethylen-lactic acid, the β- chloropropionic acid. These two bodies possess such different properties, that they may be at once and with certainty recognised.

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* The author is indebted to Professor Sharpey of London for an account of some experiments made with this poison in 1862. The results mentioned in the above abstract harmonise in the most satisfactory manner with those obtained by Professor Sharpey.

† Berichte der Deutschen Chem. Ges. 1869, 620.

The task, therefore, which I set myself, was, by the assistance of the chlorinated acid, to determine the position in the molecule of the alcoholic hydroxyl. Thebo-lactate of lime, dried at 150°, was treated with perchloride of phosphorus in the proportion of two molecules of the latter to one of the former. This mixture was heated in a retort, attached to the lower end of a Liebig's condenser, until the disengagement of hydrochloric acid ceased, when the condenser was reversed and the volatile products distilled off. By this means the decomposition is so complete that the residue, consisting of chloride of calcium, may be heated until the glass of the retort softens without carbonising to any very sensible extent. The distillate was separated by rectification up to 111° into a residue, which did not distil without partial decomposition, and a distillate. The latter was treated with the necessary precautions* with water, to obtain the chlorinated acid, and the former with absolute alcohol, to obtain its ether.

The acid thus obtained possessed all the properties of that formed from ordinary lactic acid. A chlorine determination gave 32·95 per cent. chlorine. The theoretical amount calculated from the formula $C_3H_5ClO_2$ is 32·72. Its specific gravity is 1·27, against 1·28 found for the acid derived from ordinary lactic acid. It passed entirely between 185° and 186°; the boiling point of α- chloropropionic acid is 186°. The two acids have also the same outward appearance, being colourless, uncrystallisable liquids, possessing the same smell, and exercising the same corrosive action on the skin, unaccompanied by pain or blisters.

The ether also possesses exactly the same properties as that prepared from ordinary lactic acid. A chlorine determination gave 26·34 instead of 26·01 per cent. demanded by the formula $C_5H_9ClO_2$. They both boil at 144°, and have the same smell; they are also both formed with great ease by heating their acids with alcohol and sulphuric acid.

It is thus evident that the chlorinated acids obtained by the same means from the two acids under comparison are identical. The chlorine, therefore, in both cases, is united to the same carbon atom, and consequently the acids, in which this chlorine is replaced by hydroxyl, have this last named group attached to the same carbon atom, and are therefore identical.

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* Compt. Rend. lxvi. 1157.

It is proper to mention that all the above experiments on thebo-lactic acid were repeated with ordinary lactic acid, and with uniformly identical results.

The copper salts of thebo-lactic and of ordinary lactic acids were prepared side by side, as nearly as possible under the same conditions, and in similar vessels, and on comparing the two salts, it was impossible to detect the slightest difference in their crystalline form. The free acid in concentrated solution produced no effect on the plane of polarisation of light.

I am engaged at present on the further comparison of the acids, and hope to have the honour of communicating my results to the Society on a future occasion.

In concluding, I take this opportunity of expressing my best thanks to the Messrs Smith, who in the most liberal manner placed at my disposal a large quantity of perfectly pure thebo-lactate of lime.

6. On the Bones of a Seal found in Red Clay near Grangemouth, with Remarks on the Species. By Professor Turner.

Towards the end of last autumn, one of my pupils, Mr William Stirling, B.Sc., requested me to determine some bones which had been found whilst sinking a new shaft for a pit in the Grangemouth coal-field. On examination, I found these bones to be the two halves of the lower jaw, a fragment of the upper jaw with some loose teeth, the right temporal bone, the atlas with fragments of other vertebræ, the glenoid part of the left scapula, the right astragalus and femur, and small fragments of other bones of the skeleton of a seal. The animal had not reached the adult state, for the epiphyses of the femur were not united to the shaft. The bones were imbedded in a stiff red clay.

Early in the present year, I was informed by Mr Stirling, the manager of the Grangemouth collieries, that Mr Burns, of Glasgow, had obtained some seal's bones from the same locality, and had exhibited them at a recent meeting of the Geological Society of Glasgow. Through the courtesy of Mr Geikie and Mr Croll of the Geological Survey, I have had the opportunity of examining the bones obtained by Mr Burns, which undoubtedly formed a part of the skeleton of the animal, some of the bones of which Mr Stirling had previously given to me, for I found amongst them the missing condyloid epiphysis of the right femur. These consist of one of the cervical, and of fragments of other vertebræ, of portions of the ribs, of the left occipital condyle, of a portion of the innominate bone and acetabulum, and of digital bones, more especially the terminal phalanges.

On a visit to the locality a few weeks ago, Mr Stirling gave me the following particulars :—

In the summer of last year a new shaft was sunk on Towncroft Farm, Grangemouth, to reach the coal in that district.* In the course of the operations the following strata were bored through :—

	ft.	in.
Surface soil,	4	0
Gravel sand,	0	9
Blue mud and sand,	16	0
Channel bed,	4	0
Sand and water,	8	0
Red clay mixed with sand,	11	0
Pure red clay,	36	0
Soft blue till,	38	0
Red sand,	1	0
Blue till,	5	0
Sand,	1	0
Hard blue till,	31	0
	155	9

The hard blue till lies on the rock in which the coal occurs.

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* In a paper read before the Geological Society of Edinburgh, May 1869, and published in their Transactions, Mr Jas. Croll has given an account of the geology of this district; and in a paper read before the Geological Society of Glasgow, April 2, 1868 (Transactions, iii. p. 133), Mr Jas. Bennie has recorded the results obtained in the course of "boring" operations in the valley of the Clyde near Bowling, the haugh of Balmore, the valley of the Kelvin, and round by the south-eastern end of the Campsie Hills into the valley of the Forth, near Grangemouth, which reveal that "a great deep hollow stretched from sea to sea, fairly splitting Scotland in twain."

Whilst removing the blue mud and sand, superficial to the channel bed, the lower end of the left humerus of a large red deer was met with.

The seal's bones were found near the bottom of the pure red clay, at a depth of nearly 80 feet from the present surface of the soil, and nearly 68 feet below the present sea-level. The shaft of the pit is 530 yards distant from the Carron river to the south, and 1680 yards from the estuary of the Forth on the east.

That bones of a species of seal have occasionally been found imbedded in clay, in the middle district of Scotland, is a fact well known to naturalists. But the relations which these bones had to the surface, and to the present sea-level, differ in some important particulars from those of the Grangemouth seal.

In 1825, Dr Knox* directed attention to the bones of a seal found near Camelon, in a bed of clay 90 feet above the present level of the Forth. Dr David Page described† and presented to the Museum of Natural History in this city the almost perfect "skeleton of a seal, found in the Pleistocene clays of Stratheden," 150 feet above the present sea-level, about 16 feet from the surface of the soil, and about 5 miles inland from the influence of the tides.‡ Dr Allman on two occasions § exhibited to this Society bones of a seal—in the one instance, obtained from the Tyrie clay-field, Kirkcaldy, 30 feet above the present sea-level, 18 or 19 feet from the surface of the soil, and a quarter of a mile from the shore of the Forth; in the other instance, from the clay-field at Portobello, about 20 feet above the present high-water level, and about 15 feet below the surface of the soil. The Rev. Thomas Brown showed to this Society* portions of the skeleton of a seal, obtained from a brick-field at Errol, 45 feet above the present sea-level, and about 1½ mile from the estuary of the Tay. The bones were well imbedded in the brick clay, which also contained shells such as are now found in the polar seas, and which testify to the arctic rigour of the climate at the time when the clay was deposited.

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* Memoirs of Wernerian Society, v. 572.

† Proc. British Association, Sept. 1858.

‡ Since my paper was read to the Royal Society, Dr Page has informed me that he obtained a second young seal's skeleton from the Stratheden clay, which is now in the Museum of Natural History, St Andrews. Nearly perfect skeletons of the surf and eider ducks, *Oidema* and *Somateria*, were found in the same clay. Dr Page also tells me that he has obtained seal's bones from the brick clays at Garbridge and Seafield, near St Andrews; from a brick-field at Dunbar; and from brick clay at Invernetty, Aberdeenshire. These clays are in the same horizon as the Stratheden clay. I find also that the skeleton of the young seal, in the St Andrew's Museum, has been carefully described by Mr R. Walker (*Annals and Magazine of Natural History*, Nov. 1863). He shows clearly that it is not *Callocephalus vitulinus*, and he considers it to be a young individual of *P. groenlandicus*. I have not yet examined this specimen.

§ Proc. Roy. Soc. Edinburgh, April 19, 1858, and March 21, 1859.

As to the character of the clay in which the bones of the Grangemouth seal were found, Mr Peach, who has surveyed the district, and Mr Geikie, and Mr Croll, pronounce it to have been deposited under decidedly arctic conditions. Mr Peach also tells me that the Grangemouth clay is continuous with that at Camelon, near Falkirk, where the seal's bones which Dr Knox examined were found, and that it possesses the same characters as the Stratheden clay, in which lay the skeleton of the seal described by Dr Page.

Mr David Robertson of Glasgow has also examined the Grangemouth red clay with reference to the occurrence in it of minute organisms. He reports that he has found two species of Foraminifera, *Polymorphina compressa* (D'Orb) and *Nonionina asterizans* (F. & M.), and one species of Ostracoda, *Cytheropteron montrosiensi.* This Ostracod Mr Robertson states to be common in the brick clays of Annochie, Dryleys, Errol, Elie, and Bannie on the east of Scotland, which deposits contain arctic shells not now living on the British coasts.

Mr Bennie also informs me that Mr Robertson has obtained from the muddy sand and fine sandy clay which overlie the Grangemouth pure red clay, fragments of shells, the *Tellina balthica*, a shell which, Mr Jeffreys states, agrees exactly with similar fragments found by Professor Lilljeborg at Upsala. No fragments of shells have as yet been found in the red clay itself. The geological evidence is in favour of the view that the Grangemouth clay is glacial, and belongs to the same class as other undoubtedly glacial clays on the east coast of Scotland. The difference in the relation to the present sea-level between the Grangemouth clay and the other clays presents no difficulty in placing them in the same category; for we have but to suppose that, during the period of submergence, when these clays were formed, the water in the Grangemouth locality was some 200 feet deeper than in the districts of Stratheden or of Errol, so that the change in the relative position of land and water which has taken place since that time has caused the Stratheden clays to be elevated 150 feet above the present sea-level, whilst the Grangemouth clay is some 60 or 70 feet below it.

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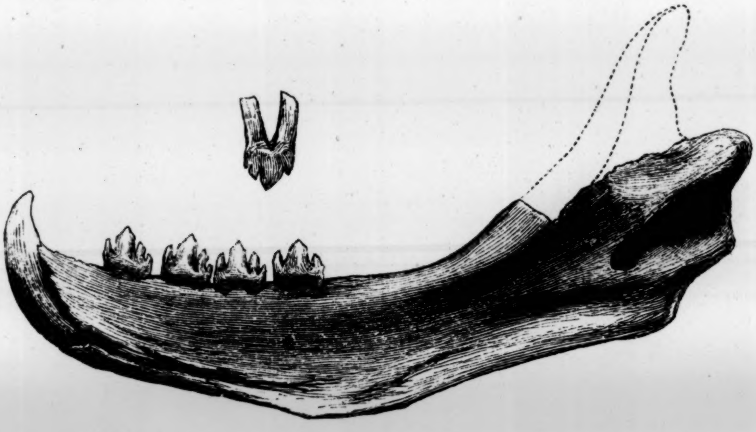
* Trans. Roy. Soc. Edinburgh, xxiv. p. 629.

I shall now proceed to inquire into the characters of the bones of the Grangemouth seal, with the view of determining—1*st*, Whether the animal was of the same species as the seals whose bones have been found in beds of clay in Scotland by other naturalists; and, 2*d*, Whether the species is or is not the common seal, *Callocephalus vitulinus*, which now frequents our coasts.

With regard to the first part of the inquiry, I have compared this Grangemouth seal with the Errol seal found by the Rev. Thomas Brown, with the skeleton from Stratheden, and with the bones of the Portobello seal, which form a part of the natural history collection in the Museum of Science and Art. I may mention, that the bones from Portobello have received some important additions since Dr Allman drew attention to them at the meeting of this Society; for Dr Andrew Balfour, by whom they were discovered, has added to the collection one-half of the lower jaw and several teeth.

As regards the Errol seal, the bones recovered were vertebræ and ribs, of which two only, viz., the atlas and one of the lower cervical vertebræ, have representatives in the Grangemouth skeleton. The Errol seal is an older animal, and the bones are larger and more completely ossified than those of the Grangemouth seal; but when due allowance is made for the difference in age, their form and general characters are so much alike that I believe them to be animals of the same species. The materials for comparison with the Portobello and Stratheden seals are, fortunately, more complete; for in them, as in the Grangemouth seal, the lower jaw and teeth are almost perfect, and the femur, scapula, and other bones are represented in each skeleton. All three animals were immature, for the epiphyses of the thigh bones are not yet anchylosed to the shafts. The atlas of the Portobello seal is somewhat less in its antero-posterior diameter than in the one from Grangemouth, and the distance of the inferior dental foramen from the hinder end of the lower jaw is greater in the Portobello and the Stratheden than in the

Grangemouth specimens. These differences are, I believe, merely individual and not specific. On the other hand, there is so close a correspondence in the general form of the lower jaws, in the number and cuspidation of the teeth, and in the mode in which they are implanted in their sockets, that I am of opinion these seals were animals of the same species. This identity in the specific characters of the seals found in the clay formations on the east coast of Scotland furnishes an additional argument in favour of the view, that they have been deposited at the same epoch and under the same conditions. We may now inquire if this clay seal corresponds with the present British species, the *Callocephalus vitulinus*.



Inner surface of the right half of the lower jaw of the Grangemouth seal, the size of nature. The outline of the coronoid process is filled in from the Portobello seal. The single tooth is one of the upper molar series.

Dr Knox stated that the Camelon seal was identical with the species now inhabiting the Forth, and many other naturalists who have written on this matter are inclined to the same view. At the time when Dr Knox wrote, the specific differences between the various northern seals had not been precisely made out, and the determination is even yet one of much difficulty, unless the skulls and teeth can be compared with each other. Dr Knox does not say what the bones were which came under his observation, so that we have now no means of knowing how far he had in his possession the materials for making an exact comparison.

Dr Page expresses himself with more reserve regarding the Stratheden seal. He looks upon it "as a pretty widely divergent variety of the common seal, if not a distinct species—a point, however, which yet awaits the precise determination of the comparative anatomist."

I have now carefully compared the jaws (more especially the lower, which are best preserved), and the teeth of the Grangemouth, Stratheden, and Portobello seals, not only with the adult skulls and teeth of the common seal, but with a young skull of that species, apparently about the same age as the fossil specimens, and I have no hesitation in saying that they are not of the same species. The number of teeth is indeed the same, but the character and mode of implantation of the molars exhibit important differences. In the clay seals, the number of cusps in the premolar and molar series does not exceed four, and this number is distinctly marked in all but the first and last. The second cusp in each tooth is the largest, but it does not preponderate very greatly over the first and third cusps, and the bases of the crowns are not much swollen. The teeth are set in the jaw in longitudinal series, one directly behind the other.

In the young of the common seal the cuspidation of the lower molars is not so uniform as in the clay seals; the last molar has four cusps, the penultimate has five, and the third and second only three. One cusp preponderates largely over the others, and the base of the crown is swollen. The molar teeth, also, are set obliquely in the jaw, so that one tooth not only lies in front, but somewhat to the outer side of the one behind it. This oblique setting of the grinders is also seen in well grown specimens.

The upper molars in the clay seals are smaller and more delicately formed than in the common seal. They are, as a rule, tricuspidate, and with, as a rule, the central cusp the largest. They are not set obliquely, and the more anterior do not overlap those which lie behind. In the common seal, again, the anterior cusp is usually the biggest, and the upper, like the lower molars, are set obliquely.

I have also compared the jaws and teeth of these clay seals with the skulls of *Phoca barbata*, *Halichoerus gryphus*, and *Pagophilus groenlandicus*, northern seals, which possess the same general dental formula. With *barbata* and *gryphus* there are so many points of difference that I cannot regard them as identical. With the Greenland seal, again, the points of resemblance are, in some respects, very striking. They agree in the number, mode of arrangement, and relative size of the cusps, and in the mode in which the teeth are implanted in the jaws, though the teeth are set closer together in the fossil than in the Greenland species. Unfortunately, I have not had access to a young skull of the *Pagophilus groenlandicus*, or to an adult clay seal, so that the materials for comparison have not, in this respect, been as perfect as to enable me to identify the species with accuracy. The examination, however, which I have made, leads me to think that these young clay seals may be either immature specimens of the *Pagophilus groenlandicus*, or of a closely-allied species. But it will be difficult to express a positive opinion until adult skulls are compared with each other, and the skulls of the clay seals be compared with the crania of *Pagomys fœtidus*, crania of which are not yet in my possession.

Addendum, March 12th.

Since this paper was read to the Society, I have received some additional material of considerable importance in connection with the determination of the species of seal found in the glacial clay-beds of Scotland. Dr Howden has kindly sent me the bones of an adult seal, found in glacial marine clay at Puggiston, three miles from Montrose.* Through Mr William Livesay and Dr M'Bain, I have had the opportunity of examining three crania of the small arctic seal, *Pagomys fœtidus*, Gray (*Phoca hispida*, Cuvier). These skulls were from two adult and one young specimen.†

The bones from Montrose included several vertebræ and ribs, pelvis, scapulæ, and the long bones of the extremities, together with the two halves of the lower jaw and the left upper jaw. They were found thirty feet below the surface, about three quarters of a mile from the tidal estuary of the South Esk, and about five feet above the present sea-level. I have compared these bones with the corresponding bones in the skeleton of the common seal, and have satisfied myself that they belong to animals of different species. I have also compared them with the bones of the other clay seals already referred to, and am of opinion that the Montrose seal is an adult of the same species as the Stratheden, Portobello, and Grangemouth specimens. Comparing the lower jaw of the Montrose seal with that from Grangemouth, depicted on page 110, we find that they have the same general form, differing from each other only slightly in size; that the teeth have the same characters, and are implanted in the jaw after the same manner.

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* The geology of this district has been carefully described by Dr Howden in the Trans. Ed. Geolog. Soc. 1867-68.

† These skulls were procured in the Spitzbergen seas during the arctic expedition conducted last summer by Mr Lamont.

If we compare the lower jaw of the adult Montrose clay seal with that of an adult *Pagophilus groenlandicus*, we find important differences in size, which are expressed in the following table, the dimensions being taken in straight lines—

	Clay seal.	*P. groen.*
Length from posterior border of condyle to socket of canine tooth, ...	4·2	5·1
Vertical diameter of horizontal ramus opposite last molar, ...	0·8	1·0
Antero-posterior diameter of ascending ramus just above the tubercle, at the angle,	1·1	1·6
Vertical diameter of ascending ramus, .	1·6	2·4

On the posterior border of the ascending ramus of the lower jaw of *P. groenlandicus*, a large triangular tubercle projects obliquely backwards and inward; in the clay seals, both adult and immature, the corresponding tubercle is not triangular, and has the form of an elongated almost vertical ridge. The teeth in the adult clay seal are set more closely together than in *P. groenlandicus*, and though the cusps in the fossil are considerably worn, yet there is not that preponderance of the central cusp over the anterior and posterior cusps in the fossil, as in the Greenland seal. The comparison of the temporal bones, and of the upper jaw with its teeth, of the adult fossil with the Greenland seal also showed important differences, so that I am constrained to give up the idea, at one time thought probable, that these seals were of the same species.

I have now instituted a comparison between the lower jaws of the adult clay seal and of the *Pagomys fœtidus*, and find they correspond much more closely, not only in form, but in dimensions. The corresponding dimensions of the latter to those of the clay seal already given in the table, being respectively 4 inches, 0·8 inches, 1·2 inches, and 1·4 inches; the differences, therefore, being so trifling as to be merely individual. They both possess the elongated ridge-like tubercle on the posterior border of the ascending ramus, and a deep masseteric fossa on its outer surface, which is bounded posteriorly by a ridge ascending to the outer end of the condyle, which ridge becomes continuous with that on the posterior border already referred to; in both the lower border of the horizontal ramus is incurved opposite the last molar tooth, behind which incurved portion it sweeps backwards and outwards in a graceful curve; in both the arrangement and cuspidation of the teeth are closely similar, although the intervals between the anterior molars are somewhat greater in *P. fœtidus*, than in the fossil.

The upper jaws and temporal bones in the two seals closely correspond in form.

The affinity, therefore, of the fossil seal to *Pagomys fœtidus* is very close,—so close, indeed, that I should not consider myself justified in pronouncing them to be distinct species.

So far, then, as I have had access to materials for comparison, I am inclined to think that the seal, the remains of which are found in the brick-clays of Scotland, corresponded with the now existing small arctic seal, *P. fœtidus*.

I am not aware that there is any satisfactory evidence to show that this northern seal ever visits our shores at the present day, so that we may consider the determination of its bones in the brick-clays to be an additional piece of evidence to those advanced from other data, that at the time when these clays were deposited an arctic climate prevailed over Scotland.

The following Gentleman was elected a Fellow of the Society:—

Dr J. Warburton Begbie, F.R.C.P.E.

Monday, 7th March 1870.

WILLIAM FORBES SKENE, Esq., Vice-President, in the Chair.

The following Communications were read:—

1. On the Rate of Mortality of Assured Lives as experienced by Ten Assurance Companies in Scotland from 1815 to 1863. By James Meikle, Esq. Communicated by Professor Tait.

The mortality of assured lives is introduced by a short statement of the process followed in the obtainment of the rate of mortality among the male population of England and Wales during seventeen years, and in which the results are compared with the rate obtained by following the same process with the male population of Scotland during ten years. A statement is given of the method employed for collecting the facts referring to assured lives, and of tabulating the results with the view of extracting, not only the total numbers entering upon and dying in each year of life, but of exhibiting the experience of several highly interesting and important sections of risks, and more especially with the view of showing the nature and benefits accruing from the assurance of selected healthy lives.

The subject generally is divided into the consideration of the mortality on *healthy* lives—males—females—and *diseased* lives.

Assured Male Lives.

In treating of healthy lives—males—a comparison is made of the actual number of deaths during each quinquennial period of life, with the number which might have died according to the Carlisle table and the Actuaries' table of 1837. The rates of mortality at each age, summed in periods of five years, are also compared. These comparisons point out that the Carlisle table exhibits a greater rate of mortality up to age fifty, and a lesser rate at higher ages than the experience of the offices; and that the Actuaries' table, at nearly all ages, is *slightly* greater than that of the Offices. A short comparison is made of the rates of mortality of male lives according to the three English life tables and that derived from the population of Scotland, already referred to, with the mortality of the selected healthy assured males of the ten Scottish offices. A very general view of the benefits of selection is thus obtained. The assurances on healthy male lives are divided into two classes—assurances *with* profits, and assurances *without* profits; the mortality of the "*with profit*" class exhibiting results in a highly favourable direction, and of the "*without profit*" class in an *un*favourable direction—the one being 10 per cent. and 7 per cent. less than the Carlisle and Actuaries' tables respectively, and the other about 12 and 13 per cent. greater.

The foregoing comparisons of the actual and computed number of deaths at each year of life are reclassified in another form, so as to exhibit the actual and computed deaths *out of the entrants at each age*, and thus show how far *one aggregate* table of mortality expresses or represents the experience of its several parts or sections. These comparisons are made with the Carlisle and Actuaries' tables, from which it will be seen that neither table accurately measures the experience of sections of entrants. Young entrants exhibit a greater mortality than estimated by either table. There is, at same time, exhibited a similar comparison of the experience of the ten offices, derived from the aggregate male lives, reapplied to the several sections of entrants, which points out in a still more marked manner the inappropriateness of *one* aggregate table of mortality to measure the experience of its sections. There is also exhibited the extent of the deviations, favourable as well as unfavourable, in *each year of the assurances*, from which it will be seen that the deviations are highly favourable during the first four years, and that after the fourth year they are almost always *un*favourable.

Assured Female Lives.

In considering the mortality of females, there is, in the first place, given a comparison of the difference between the mortality of males and of females of the population, and of the Actuaries' table of 1837, pointing out that a nearly similar relation exists between the results of these tables with that experienced between male and female assured lives in the Scottish offices, viz., a greater mortality of female life up to age forty-five. On the other hand, the male and female Government annuitants of 1829 and of 1860 exhibit a greater mortality of male life *at all* ages. An explanation of these differences is offered. A comparison is then made between the actual number of deaths and rate of mortality, of healthy assured females, with the computations according to the Carlisle and Actuaries' tables. There is, at same time, given a table, showing the favourable and unfavourable deviations of the *one* aggregate table of mortality, as a measure of the experience of sections of entrants. From this table it will be seen that neither the Carlisle nor the Actuaries' table correctly measures the mortality of female assurants under age thirty-five; and it will be inferred from the results given, that the table, based upon the aggregate experience of assured female lives, cannot measure the *aggregate* experience and at same time accurately represent the mortality of its sectional parts.

Total Lives—Males and Females.

After the usual comparisons of the actual and computed number of deaths and of the rates of mortality, according to the Carlisle and Actuaries' tables, there is given a view of the rates of mortality experienced on assurances effected *with* participation in profits and *without* participation, and an explanation is given of the reason of the greater mortality of assurances without profits, by pointing out that a very much greater mortality has been experienced on assurances (without profits) effected for temporary periods, averaging about 40 per cent. on lives under age 50. The relation of the aggregate to the sectional experience, as in the case of male and female lives separately, is shown with similar results. A very full comparison is thereafter effected between the mortality of assured lives with the mortality of the population. After comparing these aggregate experiences, a comparison is made between the rate of mortality experienced on assured lives, *excluding* the light mortality of the *first* year, first *two* years, &c., of the assurances, and the general aggregate rate of mortality of the population, with the view of pointing out, *in this form*, the relation of the mortality of assured lives, after the effects of selection have subsided, to the mortality of the population. The effect of selection is thereafter considered in its proper manner, and comparisons made between the mortality of persons in the same quinquennial period of life, but arranged according to the duration of the risks, showing that the light mortality during the years while selection is in operation is balanced by a heavier mortality thereafter, and showing further that that heavier mortality is considerably greater than the general average mortality of a single aggregate mortality table. These are exemplified in various forms. The comparisons, however, are all based upon lives *once* assured. There is, finally, given in one view the rate of mortality experienced on all entrants of each age, during each year of assurance, as the true exponent of the rate of mortality on assured lives, along with five abridgments of the same, in the case of persons assuring at each quinquennial age.

Causes of Death.

There is also given the intensity of the causes of death at each age, and the relation of the deaths of assured lives, from various causes, to the deaths of the male population of Scotland, pointing out the several orders of disease in which the mortality of assured lives is greater or less than the population. There is also given, in a general form, the effects of selection upon the various causes of death, pointing out those in which selection appears to have been of greatest benefit.

Diseased Lives.

The usual comparisons are made of the actual with the computed number of deaths, and also with the rates of mortality, pointing out that the mortality on diseased lives is greater than on healthy lives by about 20 per cent. The diseased lives were thereafter broken up into sections, according to the nature of the imperfections for which the extra charge was made, and showing the rate of mortality experienced on four such classes. For two of these classes—unfavourable personal history and gout—and also for the general class of diseased lives, the law of mortality is given, as well as the annual premium for assurance of L.100 at death, showing the extra charge for such classes of lives.

Years of Life.

All the foregoing methods of comparing actual with computed results have dealt with *numbers of deaths*. A method is pointed out for making comparisons of the actual years lived, with the computed number according to any table. Examples are given in the case of entrants at age 25, 30, 35, 40, 45, and 50.

Interpolation.

A description is given of the methods of deducing, and of practically applying, two processes of interpolation. One of them is based upon the principle that the quantum of mortality in each decennial period of life, in the adjusted and unadjusted results, shall agree. The second principle is based upon a formula, which expresses the number living in the law of mortality, at any age, in terms of constants, and the complement of life at that age. The formulæ for several differences are given in both cases, and the results applied to the total assured lives in the general mortality experience of the English and Scottish Assurance Offices.

2. Notes on Indian Society and Life in the Age when the Hymns of the Rigveda were composed. By John Muir, D.C.L., LL.D., Ph.D.

(*Abstract.*)

The paper began by stating, that although the hymns of the Rigveda exhibit a simpler and less developed stage of religious belief and conception than we find in the works of the earliest Greek poets, and a system of ideas wildly diverse, both from the mythological forms and the theosophic opinions, of the later Indian pantheon, and of subsequent speculation; and although many of the customs and practices of that early age are different from those of later times, we are not to suppose that in the former period the condition of society was of a very primitive description. On the contrary, there are many signs of a considerable progress in civilisation and culture then existing. The opinion of the late Professor H. H. Wilson on this head was then quoted; and as one proof in support of the position, the variety and occasional elaborateness of the metres in which the hymns are composed was referred to.

1. Some account was then given of the country occupied by the Indians of the Vedic era—of which a considerable portion is con-

sidered to have been cultivated, though much was also covered by forests—and of their villages and cities, or fortified places, and their houses.

2. A sketch was then given of the manner in which a priest of the Vedic age may be supposed to have spent the greater part of the night watching for, and hailing, with hymns and offerings, the appearance of the several deities, the Asvins—Ushas (the Dawn), Agni (Fire), Surya (the Sun), &c., at the times when they were supposed respectively to manifest themselves.

3. The discrepant opinions of two Sanscrit scholars, Professor Max Müller and Dr Bollensen, on the question whether or not the Indians made images of their gods during the Vedic age, are adduced, but it was considered that the question is not ripe for decision.

4. It was next stated that this tract of country was divided into numerous principalities, governed by their respective kings, who appear to have lived in considerable state, and to have been possessed of a good deal of wealth, both in cattle and goods of different descriptions.

5. Reference was made to the existence of both rich and poor in the communities, and some verses, in praise of liberality to the latter, translated from the original, were read.

6. Some particulars relating to domestic relations, and life and manners, were then given. Polygamy appears to have existed, but not of course as the rule. It was considered a misfortune for a woman to grow old unmarried. Women appear, sometimes at least, to have been allowed to choose their own husbands. According to a hymn of the Atharva-veda, the remarriage of widows seems to have been permitted; and from a verse of the Rigveda, it appears probable that a widow could marry the brother of her deceased husband, when the latter had died childless. Allusions to conjugal infidelity and sexual immorality occur.

7. It was stated that considerable attention seems to have been paid to personal decoration, as reference is made, in various places, to elegance of dress, and to the use of jewels. No mention is made of cotton as a material for clothing; though, as the plant is considered to be indigenous in India, and the use of light cotton cloth seems essential to comfort in so warm a climate, it is probable that it was well known. Wool is mentioned in various places.

The hair appears to have been occasionally worn wound or braided upwards in a spiral form.

8. Barley, at least, if not wheat also, and no doubt other grains, were used as food. The flesh of kine also seems to have been eaten. Wine (from what material distilled does not appear) was drunk by people of the upper classes, contrary to the usage of the later Hindus.

9. A hymn, descriptive of the variety of men's tastes and pursuits, was given in a metrical translation, in which various professions are mentioned, viz., those of poet, priest, physician, carpenter: the construction of chariots is often alluded to; and working in iron or other metals, and in hides, must have been common, as the mention of weapons of war and other metal implements, and of leather, is constantly occurring. Weaving, too, was of course practised, and boat building understood, as boats are frequently referred to. The caste system does not seem to have been developed during the earlier part of the Vedic era; but in a few of the later hymns Brāhmans are mentioned; and in one text the names of the four castes Brāhman, Rājanya, Vaisya, and Sudra, occur in conjunction. A free translation was given of a hymn in which the Brāhmans and their observances appear to be satirised. From what precedes under head 8, it will be seen that agriculture was practised, and specific references to it, and apparently to irrigation as auxiliary to it, occur.

10. Playing at dice was a favourite amusement of the Vedic Indians, as appears from numerous texts. A hymn, in which the miseries of a gambler's life are strikingly described, was given in an English metrical dress. Gaily dressed dancers or actors are referred to as exhibiting their performances.

11. Theft and robbery are alluded to as common offences.

12. As animals, wild or tame, mentioned in the Rigveda, kine, horses, sheep, goats, dogs, deer, boars, buffaloes, apes, wolves, and lions, are adduced. Elephants, too, are alluded to in the Rigveda, certainly as wild, but whether or not as tame also is not so clear. Among birds, pigeons, falcons, vultures, ducks, swans, and quails are referred to.

13. It need scarcely be said that wars were frequent in the Vedic age. Parts of two hymns translated in prose were read—one of them in celebration of Indra's prowess, and supplicating victory, and the

second in praise of armour and the bow, &c.; and a portion of one of them was also given in verse. War chariots were in use, and banners, defensive armour, and various kinds of offensive weapons, bows and arrows, spears, &c., are referred to.

14. Finally, allusion was again made to the number and elaborateness of the metres in the Rigveda; and as regards the occasional beauty and variety of the illustrative imagery, the moral depth of many of the sentiments, and the power of observation exhibited in its contents, reference is made to the hymns to the Dawn, and to several of those adduced in the course of the paper. In a few hymns we find the beginning of speculation on the origin of all things. One of these was communicated, rendered into English verse.

The following Gentleman was elected a Fellow of the Society:—

JOHN WINZER, Esq., Assistant Surveyor, Civil Service, Ceylon.

Monday, 21*st March* 1870.

The HON. LORD NEAVES, Vice-President, in the Chair.

The following Communications were read:—

1. On the Lake Basins of Eastern Africa. By Keith Johnston, Jun., Esq., F.R.G.S.

1. *Livingstone's Recent Discoveries.*

In 1866 the indefatigable Dr Livingstone is again in Africa, with the determination of filling up the great gaps in our knowledge of the lake region from Nyassa to Tanganyika, beginning the great journey from which he has not yet returned.

News arrived in England, in September 1866, that the traveller had, for a third time, entered the Rovuma river, and had succeeded in penetrating for 130 miles from its mouth, where he had found a friendly chief, whose residence he intended to make the starting-point of his expedition to the northern end of Nyassa, and the south of Tanganyika. A long period of silence then intervened, during which we were ignorant of the whereabouts of the traveller, till a report was brought to the east coast by some lying Johanna men who had deserted him, that Livingstone had been murdered near the south end of the lake. This report, however, was discredited by the head of the Royal Geographical Society, and a boat expedition sent out by the Society, under the leadership of Mr Young, confirmed the opinion of its untruth.

From his more recent letters, we learn that Livingstone passed round the southern end of Lake Nyassa, where he seems to have struck into nearly the old route of Lacerda and Monteiro, along the water parting between the tributaries of the Zambezi and the Nyassa.

Passing at a distance of about twenty miles to westward of Chinyanga, the furthest point which he had reached in his excursion of 1863 from Nyassa, he got into the valley of the Loangwa or Arangoa. The greater part of Livingstone's subsequent route is contained in his letter of date July 1868. In this he says—"Leaving the valley of the Loangwa, which enters the Zambezi at Zumbo, we climbed up what seemed to be a great mountain mass, but it turned out to be only the southern edge of an elevated region, which is from 3000 to 6000 feet above the sea. This upland may be roughly stated to cover a space south of Tanganyika of some 350 miles square. It slopes to north and west, but I have found no part of it under 3000 feet of altitude. The country of Usango, situated east of the space indicated, is also an upland. . . . Usango forms the eastern side of a great but still elevated valley. The other, or western side, is formed by what are called the Kone Mountains, beyond the copper mountains of Katanga."

Livingstone continues—"The southern end of the great valley, enclosed between Usango and the Kone Mountains, is between 11° and 12° south. In 11° 6′ south, ascending from the valley of the Arangoa, we were fairly on the upland." This was perhaps in January 1867, or about the middle of the rainy season here. He writes—"As we advanced, brooks, evidently perennial, became numerous. Some of these brooks went eastward, to fall into the Loangwa; others went north-west, to join the Chambeze." The Chambeze, with all its branches, flows from the eastern side into the centre of the great upland valley, "which," says Livingstone, "is probably the valley of the Nile. It is an interesting river, helping to form three lakes, and changing its name three times in the 500 or 600 miles of its course. I crossed the Chambeze in 10° 34′ south, and several of its confluents, north and south, quite as large as the Isis at Oxford, but running faster, and having hippopotami in them."

Livingstone reached a place called Bemba, on the plateau, in February 1867, and fixed its position in 10° 10′ south, 31° 50′ east. Proceeding northwards, in April 1867, he discovered Lake Liemba. It lies in a hollow, with precipitous sides 2000 feet down, on the northern slope of the upland. "It is extremely beautiful, sides, top, and bottom being covered with trees and other vegetation. Elephants, buffaloes, and antelopes feed on its steep slopes; whilst hippopotami, crocodiles, and fish swarm in the waters. It is as perfect a natural paradise as Xenophon could have desired. On two rocky islands men till the land, rear goats, and catch fish. The villages ashore are embowered in the oil palms of the west of Africa."

"Four considerable streams flow into Liemba, and a number of brooks, from 12 to 15 feet broad, leap down the steep bright red clay, such are the rocks, and form splendid cascades, that made the dullest of my attendants pause and remark with wonder."

Livingstone does not give any note of the direction of these four rivers, which flow into the lake; but it appears a necessary conclusion, from its position, that these should have their rise on the higher side of the plateau, and flow to the lake from the east.

"The lake is from 18 to 20 miles broad, and from 35 to 40 miles long. It goes off to north-north-west, in a river-like prolongation, two miles wide—it is said to Tanganyika." Livingstone continues —"I would have set it down as an arm of Tanganyika, but that its surface is 2800 feet above the level of the sea, while Speke makes the lake Tanganyika 1844 feet only." The observation of Livingstone here confirms the opinion of Mr Findlay, given in an able paper read before the Geographical Society in 1867, in which, by a recomputation of the thermometer heights measured by Captain Speke, he came to the conclusion that Tanganyika Lake was at an elevation of 2800 feet above the sea; and that, since its fresh waters must have an outlet, this would most probably be found to be to northward.

Livingstone continues—"I tried to follow this river-like portion of Liemba, but was prevented by a war which had broken out between the chief of Itawa and a party of ivory traders from Zanzibar. I then set off to go 150 miles south, then west till past the disturbed district, and to explore the west of Tanganyika, but, on going 80 miles, I found an Arab party, showed them a letter from the Sultan of Zanzibar, which I owe to the kind offices of his Excellency Sir Bartle Frere, late governor of Bombay, and was at once supplied with provisions, cloth, and beads. . . . After peace was made, I visited Nisama, the chief of Itawa, and having left the Arabs, went on to Lake Moero, which I reached on the 8th September 1867. In the northern part Moero is from 20 to 33 miles broad. Further south it is at least 60 miles wide, and it is 50 miles long. Ranges of tree-covered mountains flank it on both sides, but at the broad part the western mountains dwindle out of sight."

Lake Moero is the central one of the three on the Chambeze river. The river runs into Lake Bangweolo, at the head of the valley, and on coming out of it assumes the name of Luapula. The Luapula flows down north, past the town of the Cazembe, and 12 miles below it enters Lake Moero. Passing up the eastern side of Moero, Livingstone came to the Cazembe's town. It stands on the north-east bank of the lakelet Mofwe. This is from 1 to 3 miles broad, and nearly 4 long. It has several low reedy islands, and yields plenty of fish, a species of perch. It is not connected with the Luapula or Moero.

"I was forty days at Cazembe's," says Livingstone, "and might then have gone on to Lake Bangweolo, which is larger than either of the other lakes, but the rains had set in, and this lake was reported to be very unhealthy. I then went north for Ujiji, where I have goods, and I hope for letters; for I have heard nothing from the world for more than two years; but when I got within thirteen days of Tanganyika, I was brought to a standstill by the abundance of water in front. A native party came through and described the country as inundated, so as to be waist deep, with sleeping places difficult to find. This flood lasts till May or June. At last I became so tired of inactivity that I doubled back on my course to Cazembe."

In this attempt to reach Ujiji, Livingstone appears to have intended to reach the west side of the Tanganyika by the road which Captain Speke reported from Warruwa (evidently the Rua of Livingstone) to the ferry by which he had crossed from Ujiji; and it was apparently during this attempt that Livingstone obtained, by actual observation, the report which he gives of the lower course of the Luapula. He says—" On leaving Moero at its northern end, by a rent in the mountains of Rua, the river takes the name of Lualaba, and passing on north-north-west forms Ulenge in the country west of Tanganyika. I have only seen it where it leaves Moero, and where it comes out of the crack in the mountains of Rua."

The flat inundated country beyond this point seems to have been his turning-point. He says—" To give an idea of the inundation which, in a small way, enacts the part of the Nile lower down, I had to cross two rivulets, which flow into the north end of Moero—one, the Luo, had covered a plain abreast of Moero, so that the water on a great part reached from the knees to the upper part of the chest. The plain was of black mud, with grass higher than our heads. We had to follow the path which the feet of passengers had worn into deep ruts. Into these places we every now and then plunged, and fell over the ankles into soft mud, while hundreds of bubbles rushed up, and bursting emitted a frightful odour."

Having returned to Cazembe's in about February or March of 1868, Livingstone seems to have gone south at the beginning of the dry season, to Lake Bangweolo, from which his letter is dated in July 1868.

The next news we have of the great traveller is in a letter from Ujiji, on Lake Tanganyika, dated May 1869. He appears to have reached this point by the *eastern* side of Tanganyika, not by the western as before attempted; since he writes in the above letter, " As to the work to be done by me, it is only to connect the sources which I discovered from 500 to 700 miles south of Speke and Baker's, with their Nile. The volume of water which flows north from latitude 12° S., is so large, that I suspect I have been working at the sources of the Congo as well as those of the Nile. I have to go down the eastern line of drainage to Baker's turning-point. Tanganyika and Nyige Chowambe (Baker's ?) are one water, and the head of it is 300 miles south of this. The western and central lines of drainage converge into an unvisited lake west or south-west of this. The outflow of this lake, whether to Nile or Congo, I have to ascertain." From the above it would appear that Livingstone had made an excursion northward from Ujiji, either by land or on the lake, to ascertain the union of the Tanganyika with the Albert Nyanza.

News has since been received, which shows that Livingstone was still at Ujiji in July 1869. In January of this year, a report arrived from the west coast of the continent, describing the fearful end which the traveller had come to, of his being quartered and burnt; but this report turns out to be an old story of date June 1868, with its plot laid on the Zambezi, and at this time we know that Livingstone was safe on the Chambeze lakes.

2. *The Sources of the Nile.*

The main point of interest in the latest travels of Livingstone, and that which gives to them a distinctive importance over the great accomplishments of his former journeys, is that in these Livingstone has undoubtedly visited and beheld the long sought-for sources of the Nile. It is true that there is considerable doubt as to which of the basins that he has explored will ultimately be acknowledged as the cradle of the Nile; but this at least is certain, that the real head streams have been visited by Livingstone, and the long-vexed question has, by these last explorations, resolved itself into a choice between two or perhaps three main streams.

Livingstone himself has apparently no bias in favour of one or other, so that the discussion is a perfectly open one. The three rival head streams are—first, the feeders of Lake Liemba; and, second, the Chambeze and its lake chain, both of which rise near the eastern edge of the great longitudinal plateau of the side of Africa next the Indian Ocean; the third is the source recently claimed for the Nile by Dr Beke, in his "Solution of the Nile Problem,"* the Great Casai or Kassabi river, which rises nearer the Atlantic side, in 12° S. Of the first of these, the feeders of Lake Liemba, we may say, with almost absolute certainty, that they are tributaries to the Nile, and it is most probable that they are the *sources* of that river. Livingstone has found these rivers flowing into Lake Liemba; a river-like prolongation unites Liemba and Tanganyika, these two appearing to be at the same level; then Tanganyika and Nyige Chowambe, which is evidently the Albert Nyanza, are one water, and that the last is a reservoir of the White Nile is undoubted.

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* Athenæum, February 1870.

The union of the second presumptive head stream, the Chambeze, with the Nile, is less apparent; indeed, the balance of evidence seems to show that it must be the head of the other great river of Africa, the Congo. If the Chambeze prove to join the Nile, then the streams to the Lake Liemba become mere tributaries, since the course of the Chambeze is by far the longer of the two. The feeders of Liemba and the Chambeze rise, however, side by side, on the eastern plateau. The Chambeze flows down into the central valley through Lake Bangweolo, and then northward through Lake Moero. Livingstone describes Lake Moero as beginning 12 miles below the position of the town of Lunda, the capital of the Cazembe (lat. 8° 40′ S., long. 28° 20′ E.), whose position may be laid down with tolerable accuracy from the former journeys of the Portuguese travellers. Since Livingstone proceeded north from Cazembe's town, along the eastern shore of Moero, in his attempt to reach Ujiji in 1867, the great bulk of this lake must lie to the westward of the meridian of Lunda, or about 120 miles to westward of Tanganyika. Dr Livingstone has seen the river at its outflow from the lake, and also at the point where it emerged from the crack in the mountains of Rua, when, according to his own observation, the river turned to north-north-west to form Ulenge, a third lake or marsh in the country west of Tanganyika. This north-westerly turn would carry the river quite out of the direction of the Nile basin, and the higher side of the continent being to the east, the probability is, that the river continues to curve to the west.

Again, the valley of the Chambeze, in the plateau where Livingstone crossed it, is, no doubt, one of the greatest hollows in the high land, so that the height of the river bed here may be taken at 3000 feet, the lowest level of the limits which Livingstone gives to the undulation of the plateau, or only 200 feet above the level of Tanganyika. Descending into the great valley to Lake Bangweolo from the plateau, the Chambeze must have a considerable fall; from Bangweolo to Moero there must be a second descent. The Cazembe's country, which extends round to the south of Tanganyika, is described as flat, and its rivers are currentless and stagnant. If Moero were at a higher level than Tanganyika, would not the river which leaves it take a course over the level country instead of facing towards, and making its way through a crack in the mountains northward? Seeing that the river does force its way through these mountains, the presumption is, that Moero is at a lower level than Tanganyika; and if this be the case, the river which descends from it through the mountains can never again ascend to the level of the Nile lakes to join them, but must find some other course.

With regard to the third advocated source, the Kassabi river, of which Dr Beke affirms it to be his belief that it is the head stream and upper course of the Nile of Egypt, the difficulties of its joining the Nile appear to be even greater than the last. The upper course of this river only has been explored. It springs in the Mossamba Mountains, which are on the inner borders of Angola and Benguela, its sources being close to those of the Quango river, a tributary of the Congo. The Kassabi is known to flow northward as far as 8° S. to westward of the capital of the Muata Yanvo.

Dr Livingstone crossed its head on his journey from the Zambezi to Loanda; and the reports which he collected from the subjects of the Muata Yanvo's kingdom, all tend to prove, that whatever direction its middle course may take, in its lower course the Kassabi flows round to westward, and is joined by the Quango. The trader, Graca, who penetrated to the Muata Yanvo capital in 1846, says, that "the territory of this chief is shut in by the great rivers Kassabi and Lurua (a tributary of the Kassabi)." "These rivers," he continues, "flow into the river of Sena" (the Zambezi). The latter part of this statement we now know to be incorrect; but, taken as a whole, it indicates an easterly bend in the lower course of the river to enclose the kingdom of the Yanvo on the west and north, and to flow as if to the Zambezi. The Hungarian traveller, Ladislaus Magyar, has penetrated furthest of the three who have visited this region, and his information seems to agree well with this last. He reports that the Kassabi, after forming the waterfall of Muewe (in about 11° S. latitude), bends gently to northward; but further on takes an easterly direction in its lower course, and reaches a breadth of several miles at the place where it *touches upon* the extensive lake Mouva or Uhanja.

Now, if we turn the Kassabi river eastward in latitude 8° S., in agreement with the above description, we find that it meets the position which Livingstone's letters give to Ulenge, the lake or marsh to which the Chambeze river flows, and whose waters Livingstone tells us by report, in his recent letters, are *taken up* by the Lufira, a large river which, by many confluents, drains the western side of the great valley.

Is not the *Lufira*, then, the lower course of the Kassabi, and the Lake Ulenge of Livingstone, whose waters are *taken up* by the Lufira—the Uhanja lake of Magyar, which the Lower Kassabi *touches* upon?

The same difficulties which appear in the way of the Chambeze river and lake chain joining the Nile, hold also against the Kassabi, which, from the above reports, would seem to join this river at Lake Ulenge.

Next, the question arises, if these rivers do not form a part of the Nile system, where then shall we find an outlet for them? The answer to this is plainly, in the Congo river.

The Congo was described by the Jesuit missionaries, who first visited its mouth, as so "violent and so powerful from the quantity of its waters, and the rapidity of its current, that it enters the sea on the west side of Africa, forcing a broad and free passage (in spite of the ocean) with so much violence, that for the space of 20 leagues it preserves its fresh waters unbroken by the briny billows which encompass it on each side." In the introduction to his narrative of his expedition to the Congo, Tuckey says, "If the calculation be true that the Congo, at its lowest state, discharges into the sea two millions of cubic feet of water in a second, the Nile, and the Indus, and the Ganges, are but rivulets compared with it, as the Ganges, which is the largest of the three, discharges only about one-fifth of that quantity at its highest flood." This estimate is greatly exaggerated, but Tuckey actually found that this vast river has a width of two, three, or even four miles, whilst flowing with a current of two or three miles an hour (p. 342), and this not at its mouth, but inland beyond the mountainous coast regions. Such a vast river cannot be formed in a short course, but must have its rise far in the interior of the continent.

If we take the Kassabi river and its drainage to the Nile, where shall we find a sufficiently lengthened course for the Congo? Tuckey's unelaborated notes give the opinion that the "extraordinarily quiet rise of the river shows it to issue from some lake, which had received almost the whole of its waters from the north of the line;" and again, he says, "I cannot help thinking that the Congo will be found to issue from some large lake or chain of lakes, considerably to northward of the equator." The reason of Tuckey's supposition that the lakes, which evidently maintain the volume of water in the Congo, would be found north of the equator, is this, that he found the rising of the river beginning on the first days of September. At the time of his journey little or nothing was known of the times of the rainy seasons in Central Africa from actual experience. Since then the traveller Burton has told us (in his account of the expedition to Tanganyika, R. G. S. Journal, vol. xxix.), that in the latitude of Tanganyika the rain sets in at the end of August, lasting till May; and Livingstone says, in his latest letter, that he did not proceed to Lake Bangweolo from the Cazembe's capital, where he arrived about the middle of September, *because the rains had set in.* Lake Ulenge lies between these latitudes, so that the rise of the waters of the Congo on the first of September is perfectly explainable without the necessity of taking its reservoir lakes to the north of the equator; if the lakes were there, the rise of the Congo would occur at a much earlier period of the year, as we shall afterwards notice, and, indeed, the space in which Lake Ulenge lies, seems to be the only one on the continent whose rainy season would agree with the observed rise of the Congo.

3. *The Physical Features of the Lake Region and the Lakes.*

The great highlands of the world encircle and turn their steepest verge towards the Pacific and Indian Oceans; the slope is gentle towards the great plains which surround the Atlantic and Arctic Seas. Africa is no exception to this rule, since it presents to the Indian Ocean the abrupt descent of the plateau which extends along its eastern side from the Cape Colony to Abyssinia, northward. It is true that the whole of South Africa is a plateau, with a general elevation of about 3500 feet, and that the outer edges of it rise steeply from both coasts; but the eastern side is the higher of the two, and the law of a general slope towards the Atlantic is maintained on its surface. The Lake Region occupies the central part of the eastern, or higher side, of the South African plateau, and here the line of descent to the coast-land of the Indian Ocean is marked continuously from north to south; first by the southward continuation of the outer slope of the Abyssinian table-land, then near the equator by the edge on which the great mountain peaks of Kenia and Kilima Ndjaro rise; farther south by the Rubeho Mountains, up which Burton and Speke ascended to the plateau; and then by the N'jesa Mountains, which wall in the Lake Nyassa. Farther south the cataracts of the Shiré river, 35 miles in extent, show where this river tumbles over the edge of the plateau, and the Zambezi breaks through it at the narrows of Lupata. Below this steep edge the coast-land slopes in gently to the sea, and is diversified by wide plains or scattered hill ridges.

The high surface of the South African plateau inland is hollowed out in the wide high valleys which contain its lakes and great rivers. The most northerly of these depressions in the Lake Region is that of the great lake reported by the ivory trader Piaggia, who approached within 60 miles of its northern shore. This lake appears to lie in a high valley on the northern edge of the plateau of South Africa, or rather in a recess of the northern lower land, partly shut in by the slopes of the plateau southward, and the mountain range which the traveller saw rising to south-westward beyond the lake, is perhaps only the steep northern edge of the southern plateau here.

The wide depression in which the Victoria Lake lies is shut in eastward by the continuation of the Abyssinian highland into the South African plateau. This valley appears to include the basin of the Bahari N'go, which is believed to be a vast salt marsh, or perhaps a sort of backwater of the Victoria Lake, and its slope is to north-westward, towards the angle of the northern lower land which is formed by the inner side of the Abyssinian highland running north and south, and the northern edge of the plateau of South Africa, which has a direction from east to west.

Between these two northern depressions lies the deeper and narrower valley of the Nile, which contains the Tanganyika and Albert Lakes. The beginning of this depression may be said to be at Lake Liemba, which lies sunk 2000 feet down in the edge of the plateau north of Lake Nyassa; then it opens out into a wider valley to the east of Southern Tanganyika, but again closes in the northern part of that lake, and is only a little wider where the Albert Lake is sunk between the edge called the Blue Mountains and the part of the plateau which separates this depression from the higher one of the Victoria Nyanza. To south-west of Tanganyika the narrow valley of the Upper Nile appears to have an opening into that one which contains the Chambeze and its lakes, made known by Livingstone, in the low-lying country of the Cazembé.

This valley of Bangweolo and Moero Lakes seems to be most completely surrounded on all other sides; by the high plateau of Usango eastward, by a narrower portion of it called the Muchinga Mountains southward, and again by the Kone Mountains, and a broader part of the plateau, the copper country of Katanga, on the west. The only other opening or outlet into this valley is apparently the rent in the mountains of Rua, through which the river makes its escape to join Ulenge. Westward is another and wider depression —the wide high plain which forms the kingdom of the great Muata Yanvo, watered by the Kassabi River, and stretching out between the Mossamba Mountains where the river rises, and the plateau of Katanga, which separates the Yanvo's from the Cazembe's valley.

The Zambezi valley closes the Lake Region southward. The Zambezi is the exceptional river of Africa, since it breaks through the higher side of the plateau to reach the Indian Ocean. Its sources, however, seem to be on the inner side of the plateau, springing on the western slopes of the Kone Mountains, and flowing first to the south-westward. The vast basin of this river (about 568,000 square miles) is comparable to that of the Volga, and would make more than one hundred river basins such as that of the Thames.

On the west, the waterparting of the Zambezi valley at Lake Dilolo is apparently but little elevated above the plain of the Muata

Yanvo's kingdom; but as the valley descends eastward its northern side appears to rise to the range of the Kone and Muchinga Mountains, and the valley becomes deeper and narrower where it cuts through the high edge of the plateau eastward, to reach the coast.

Lake Nyassa, a tributary lake of the Zambezi, lies in a deep longitudinal hollow near the edge of the plateau, only retained by the high barrier of the N'jesa Mountains. The narrow valley of the Shiré river, which flows from it, continues this hollow to the Zambezi. Lake Shirwa is similarly situated, but has no outlet, and in consequence its waters, in distinction to the fresh sweet water of the other lakes, are brackish. The approximate area of each of the eleven great lakes of this region, so far as their extent is known, is as follows:—

	Square Miles.
Victoria Nyanza, ...	29,900
Albert Nyanza, ...	25,400
Piaggia's Lake, ...	11,000 ?
Tanganyika, ...	10,400
Nyassa, ...	8,600
Bahari N'go, ...	6,000 ?
Bangweolo, ...	3,700
Moero, ...	2,000
Ulenge, ...	1,000 ?
Shirwa, ...	800
Liemba, ...	700
	99,500

The whole extent of water surface in this Lake Region is then nearly 100,000 square miles, an area not far short of that of the British Isles. A more definite notion of the great extent of these inland seas of fresh water may perhaps be obtained, if we observe that a direct passage across the Victoria Lake, from shore to shore (in its presently believed extent), corresponds in length to a voyage across our North Sea from Hull to Rotterdam, or from the eastmost land of Scotland at Peterhead, to the Norway coast.

The more important of the rivers of the Lake Region have been noticed in speaking of the routes taken by the travellers who have discovered them. One of the main hindrances to the exploration of South Africa, is the difficulty of making use of these rivers as highways into the continent. The coast rivers of the Lake Region, or, indeed, of the whole of Eastern Africa, are barred at their mouths by the aggregated debris which they carry down, raised in banks on the coast between the downward current of the river and the opposing monsoon, or trade wind, blowing towards the coast. If this bar is passed at the mouth, still the navigation even of the largest rivers cannot cross the edge of the plateau where cataracts and rapids form a new obstruction. The vast lakes of the interior, and their great connecting rivers, however, present great lines of navigable water, which in a higher civilisation would be utilised for busy traffic, the line of the Nile basin in the Tanganyika and Albert lakes alone affording an unbroken voyage of about 900 English miles.

Piaggia, the traveller who has been nearest to the great lake which lies to the north-west of the Albert Nyanza, reports a great river called the *Buri*, flowing to westward, at some days' journey from Kifa (his furthest point), and which issues out of his great lake. The same river has been reached, at some distance from its supposed outlet, by the brothers Poncet (French ivory traders), who have long trafficked in this region, and they express the opinion that this river unites the equatorial lakes with Lake Tchad, by means of the Shari river. This they proposed to prove by an expedition on it in boats. The question, What becomes of this great river? which, at its outlet from the lake, is so large as to be only passable in boats, is an interesting one. It is certainly no tributary of the Nile, and the two most probable lower courses which it may have are those of the Shari to Lake Tchad, or of the Benue river to the Niger. If it ultimately proves to flow to Lake Tchad, it will give a striking evidence of the vast amount of evaporation which must exist in the region of that lake, since it has no outlet; but the Benue river seems to be its most probable course, for at its confluence with the Niger, the Chadda, or Benue, is the larger river. The Ogowai river is also a possible lower course for the Buri, but if the lake reported by Piaggia be, as we suppose, on or beneath the northern edge of the plateau of South Africa, it seems only natural that the river from it should seek the lower land to northward, than turn westward along the northern slope of the plateau.

4. *The Nature of the Surface of the Lake Region—Its Great Fertility.*

Africa, the only one of the continents which has a large extent of land on each side of the equator, presents a series of zones, each of which has a different nature of surface, and these belts correspond very closely with one another on the opposite sides of the equator. The central area of Africa, below the equator, in the zone of long rainy seasons, or of almost constant rain, is a region characterised by dense forests, and a most luxuriant overgrowth of vegetation, comparable to that of the selvas of the Amazon River in South America, which occupy the same equatorial position on the globe. To north and south of this forest zone is a belt of less wooded country, merging gradually into open cultivated or pasture lands. Next, these grass lands pass into the two great almost rainless deserts of the Sahara in the north, and of the Kalahari southward. Beyond the deserts, at the extremities of the continent, the outer slopes of the Cape Colony in the south, and of the plateau of Barbary, the "Tell" country, in the north, present a second zone of fertile and cultivated country.

The Lake Region extends from this central forest zone, in which the equatorial lakes are formed, through the more open belt of less wooded country southward, as far as the Zambezi valley, and this area is almost everywhere adorned with the choicest natural varieties of shady forest, with luxuriant underwood, or clumps of trees with rich grassy plains between.

5. *Climate of the Lake Region.*

Nowhere more than in this central region of Africa are the subjects of temperature, rain, and winds, more closely interwoven, or mutually dependent, the one upon the other. In the passage of this area beneath the sun, a low atmospheric pressure is produced by an ascending heat column, and by the condensation of vapour in this; the winds flow into the ascending column, and bring with them the moist air of the ocean, which, condensing in copious floods of rain, reduces the temperature, whilst causing a further opening, into which the winds blow with increased power. The area of low pressure, with its attendant circumstance of winds and rains, always tends towards that part of the continent which is vertically beneath the sun's rays, and thus moving up and down the face of the land within the tropics, gives the wet and dry, the cold and hot, seasons of the year in this region. On the coast the seasons are sharply defined: the continental and the oceanic monsoons divide the year between either a single or a double wet and dry season; but in the high interior plateau in which the lakes are situated, the winds are drawn into the pendulating area of low pressure from the ocean, nearly throughout the year, and it is only when extreme limits of the tropical zone come directly under the sun, that a higher barometric pressure, an outflow of the winds, and a consequent dry period, is experienced here.

In the coastland under the equator, the country explored by the German traveller Brenner, the mean temperature of the year is 85°·1 (mean of three daily observations), the highest observed temperature (of 92°·8) having occurred in January, and the lowest (73°·4) in May. The rainy season here sets in with the south-east monsoon in April, and lasts till the end of June. The second rainy season, which we shall notice, taking place farther south in September and October, is almost lost at the equator. The north-east monsoon brings a cloudless sky of clear blue, and begins to blow here in November, lasting till March, and in this season rain is never thought of.

At Zanzibar Island, six degrees south of the equator, the mean temperature of the year is nearly 80° Fahr., rising in January to an average of 83°, falling in July to 77°; and it has a double rainy season, a stronger in March, April, and May, when the column of low pressure has passed this latitude in moving northward; and again in a weaker in September and October, when the low pressure passes in its southward course, at which times the monsoon winds change from the north-east, blowing out of Asia towards South Africa, to the south-west, blowing from Africa towards the Asiatic continent. In the low countries, beneath the edge of the plateau, about Zungomero, Burton tells us that the rain is constant, except for a single fortnight in the month of January; at most times the sun shines through a vale of mist with a sickly blaze and a blistering heat, and the overcharge of electricity is evinced by frequent and violent thunderstorms, so that the climate of Zanzibar is equally ruled by these two great land masses. On the Mozambique coast the winds are again ruled by the African continent only, and the year is divided into a dry and wet season. From April till November the undeflected south-east trade wind blows upon this coast, and either from the lowness of the land or the shelter it obtains from the high island of Madagascar, this wind brings the dry season. From November to March the north-east monsoon, here at its furthest south limit, having passed over the warm Indian Ocean, brings the rainy season.

On the plateau inland, the climate and seasons are different. The mean annual temperature of the table-land in the neighbourhood of the Victoria Nyanza was found by Speke and Grant to be only about 68° Fahr., a temperature not greater than that of the south coasts of the Mediterranean, a climate not unsuitable to Europeans, since a hot summer in England is far more oppressive.

The rainfall in this high region is also an exceptionally small one for a tropical country, having been found to be only about 49 inches, or not so much as that of many parts of England, and this may partly be accounted for by the fact that this part of Africa is deprived of all rain from northerly winds, which come overland, and the prevailing east winds lose much of their moisture on the high eastern slopes of the plateau before reaching this region.

The traveller Burton gives an account of the very different climate of the deeper valley of the Tanganyika Lake. Here the rains divide the year into two unequal portions of eight and four months,—namely, the wet monsoon, which commences with violence in the end of August, and lasts till May, and the dry hot weather which completes the year. During the wet monsoon (1858) the prevalent winds were constantly changing. The most violent storms came up from the south-east or south-west of the plateau of Umyamwesi, to westward of the lake. Here he says that there are but two seasons, a summer and winter, and the rains begin in the middle of November. "The moisture bearing wind in this part of Africa is the fixed south-east trade, deflected into a periodical south-west monsoon." Further south in the Cazembe's country, the rainy season appears from Dr Livingstone's letter to begin in September, and he says that the floods in the country west of Tanganyika last till May or June. In the northern part of the Zambezi valley the traveller Silva Porto found the rains set in on the

Arangoa river in February, and they ended with him on the eastern side of the Nyassa in June.

On the Zambezi river in the Makololo district, Livingstone observes that the rain follows the course of the sun, since it falls first in October and November when the sun goes over this zone southward. When the tropic of Capricorn is under the sun in December, it is dry, and December and January are the months in which the droughts are most severe in the countries between the Zambezi and the Kalahari. When the sun turns again to northward in February, March, and April, the great rains of this part of the Zambezi valley are experienced.

6. *Population.*

The Lake Regions of Africa are well peopled. Behm, in his "Geographical Year-book," has estimated the population of that part of Eastern Africa, which lies between the equator, the line of Lake Tanganyika, the Cazembe's country, and the Portuguese colonies on the coast, at 3,500,000. This gives a density of population of about six to a square mile, but is apparently rather under than above the mark. It is true that the slave trade must reduce and disturb the population of this part of Africa to a great extent, since many thousands of slaves are annually brought down to and exported from the harbours on the coast; but, on the other hand, travellers in this region report a continuous population. Captain Grant describes the part of the Lake Region which he traversed as too thickly peopled to harbour many wild animals; the shores of Lake Tanganyika are, according to Speke, "thickly inhabited by numerous tribes;" and in his voyage on Lake Nyassa, Livingstone says, "Never before in Africa have we seen anything like the dense population of the shores of Lake Nyassa, especially in the south. In some parts there seemed to be an unbroken chain of villages. On the beach of well-nigh every little sandy bay, black crowds were standing gazing at the novel spectacle of a boat under sail."

The inhabitants of the Lake Region appear to belong entirely to the negro or negroid race, but are closed in to north and south by peoples of a different stamp.

The *Niam Niams* who inhabit the country north of the lake reported by Piaggia, and west of the Albert Lake, who had formerly the reputation of being "half men and half dogs, with a fan-like tail," and of having a disposition to eat their fellow-creatures, prove, on nearer inspection by the traveller Piaggia, to be men of powerful, regular, and fine figure, of stately carriage, with bronze-coloured skin, long hair, and thick beard, barbarous indeed in their customs, but not cannibals. They are considered to be identical with the interesting race of the *Fellatah*, the dominating people of the western Soudan, or are perhaps a step between these and the Gallas of the east.

Burton describes the peoples he met with between the east coast and the Lake Region:—"The Sawahili of the Zanzibar coast are sprung from the intercourse of foreign traders and emigrants, Phœnicians, Jews, Arabs, and Persians, with the African aborigines. The Balonda people of the kingdom of the Muata Yanvo, to the west of Lake Tanganyika, are almost pure negroes; and between these and the mixed east coast there is a tolerably regular gradation of negroid races from east to west, brought about partly by long intercourse with foreign settlers, and in part by intermixture with the non-negro races of North Africa. The high road from the coast to Ujiji runs through comparatively quiet and peaceful races." "Cannibalism," says Burton, "is rare in Eastern Africa, and results either from policy or necessity."

The aspect of the great mass of this negroid race is not unprepossessing. They are tall and well-made mulattos, rather above the European standard. A giant or a dwarf is never seen. The people of the maritime regions have rough dirty skins of a dull pale black, like that of diluted Indian ink; from the central elevation of the eastern plateau the complexion improves, and further inland the yellow skin, so much prized in Eastern Africa, appears. From the Unyamwesi plateau to Tanganyika Lake, in those lower levels where heat and humidity are in excess, the people become lamp black, without a shade of brown. The negroid races appear to extend down the outer slope of the continent to near the Zambezi valley southward.

Livingstone speaks of the *negro* peoples of the shores of Lake Nyassa; and Silva Porto describes the natives he met with in the northern watershed of the Zambezi valley as "hospitable negroes."

The River Zambezi is nearly the boundary between the negroes or negroids of the Lake Region, and the Kaffir races of South Africa.

South of the Zambezi the kingdom of Mosilikatze has been made up of the remains of a number of formerly independent tribes conquered by the Matebele Kaffirs pushing northwards; and Sekeletu's Makololo kingdom, in the Upper Zambezi valley, was founded by a former ruler who led this conquering Kaffir tribe from the head of the Orange river northward, and incorporated the vanquished tribes with this one to form his kingdom.

The most important kingdom of South Africa is the empire of the Muata Yanvo, whose subjects are purely negroes. The dominion of this potentate seems to reach from the Mossamba Mountains, at the head of the Kassabi river westward, to the town of Shinte, on the Leeba river, and the Muchinga Mountains southward, and thence round to the southern part of the Tanganyika Lake.

The northern extent of this kingdom is as yet unknown. The Muata Yanvo's empire includes that of the Cazembe, who is his vassal, and who rules for his sovereign over that part of the kingdom which is separated from the main portion by the desert or mountainous country of Katanga. The fertile and thickly peopled area, known to be under the sway of this great Central African ruler, is far greater than any of the kingdoms of Western Europe, and might be compared in extent to the united bulk of France and Italy.

In conclusion, we may glance at the enormous labours of the great traveller Livingstone, to whom the world is indebted for so vast a portion of its knowledge of the African continent, and whose recent travels have given a fresh interest to this part of the globe. The area of South Africa, which Livingstone has already explored, and not only explored, but in great part surveyed with accuracy, has an extent of about one million of square miles. It is difficult to form a correct notion of the space covered by such an area; and it may help to give an idea of the work which has been accomplished, if we remember that the united areas of all the western kingdoms of Europe—France, Austria, Germany, Italy, Spain—would scarcely make up the extent of land which Livingstone has virtually added to the known world.

2. On the Steady Motion of an Incompressible Perfect Fluid in Two Dimensions. By Professor Tait.

While discussing some of Mr Smith's applications of Maxwell's ingenious idea of representing galvanic currents by the motions of an imaginary fluid (*ante*, p. 79), I was led to the present investigation. I have since found that, as was only to be expected, I had been anticipated in a great many of the results I obtained—especially by Stokes, in the *Trans. of the Cambridge Phil. Soc.* 1843. Still it appears to me that I have a few novel results to communicate.

If ψ = const. be the equation of a current-line, Stokes has shown that—

$$\frac{d^2\psi}{dx^2} + \frac{d^2\psi}{dy^2} = f(\psi),$$

where f is an arbitrary function.

By the integration of this equation various singular results are obtained, especially as to the nature of the families of curves which can be lines of flow.

The equation of lines of equal pressure is then formed, and from it corresponding results are derived. A curious result is obtained when the motion is irrotational; in which case there is a velocity-potential ϕ, and we have—

$$P = \frac{2p}{\rho} + C = \left(\frac{d\phi}{dx}\right)^2 + \left(\frac{d\phi}{dy}\right)^2,$$

$$\frac{d^2\phi}{dx^2} + \frac{d^2\phi}{dy^2} = 0.$$

Here the elimination of ϕ gives us—

$$\frac{d^2 \log P}{dx^2} + \frac{d^2 \log P}{dy^2} = 0.$$

The method is also applied to certain cases of motion which, though not steady, can be treated as if they were steady—viz., cases in which a given state of motion is propagated in the fluid by translation or rotation; so that to a spectator moving in a given manner in a plane parallel to the fluid, the motion *appears* to be steady. Thus, for instance, we can treat as steady motion the case of two equal parallel vortex-filaments rotating either in the same or in contrary directions.

3. On the most general Motion of an Incompressible Perfect Fluid. By Professor Tait.

This is a quaternion investigation into the circumstances of fluid motion, especially with reference to the case of vortices. The method employed is very similar to that which I gave to the Society in 1862 (*Proc. R.S.E.* April 28).

It is shown that if σ be the vector-velocity of a particle of fluid, so that

$$\sigma = iu + jv + kw,$$

and if we introduce the operators D_σ and δ_σ such that

$$D_\sigma = \frac{d}{dt} + u\frac{d}{dx} + v\frac{d}{dy} + w\frac{d}{dz} = \frac{d}{dt} + \delta_\sigma,$$

together with Hamilton's operator—

$$\triangleleft = i\frac{d}{dx} + j\frac{d}{dy} + k\frac{d}{dz},$$

the equations of fluid motion and of continuity are—

$$\left.\begin{aligned} \triangleleft P - \frac{1}{r}\triangleleft p &= D_\sigma \sigma \\ S\triangleleft\sigma &= 0, \end{aligned}\right\}$$

where r is the density, and P the potential of the applied forces.

The principal transformation is effected by means of the curious theorem in kinematics

$$\triangleleft D_\sigma \sigma - D_\sigma \triangleleft \sigma = \delta_{\triangleleft\sigma}\sigma.$$

Thus, for instance, we have from the equation of motion

$$V\triangleleft D_\sigma \sigma = 0,$$

because $\triangleleft^2\left(P - \frac{p}{r}\right)$ is obviously a scalar. The above theorem then gives

$$D_\sigma \triangleleft \sigma = \delta_{\triangleleft\sigma}\sigma,$$

which proves that if $\triangleleft\sigma$ is ever zero for any particle of the fluid it must remain so for that particle.

As an additional instance of the simplicity of the method employed, the following may be given in this abstract:—

If τ be the instantaneous axis of the element of fluid, whose velocity is σ, we have—

$$\triangleleft\sigma = -2\tau.$$

But

$$S\triangleleft^2\sigma = 0,$$

whence,

$$-\frac{1}{2}\triangleleft^2\sigma = V\triangleleft\tau,$$

and

$$-\frac{1}{2}\sigma = \triangleleft^{-2}0 + \triangleleft^{-2}V\triangleleft\tau.$$

This contains the solution of the problem, treated by Helmholtz, to determine the linear velocity of each fluid particle, when the angular velocity is given.

4. Mathematical Notes. By Professor Tait.

The following self-evident propositions were employed for the deduction of several curious consequences—

(*a*.) $$4x = (x+1)^2 - (x-1)^2,$$

or, $$x^3 = \left(\frac{x(x+1)}{2}\right)^2 - \left(\frac{x(x-1)}{2}\right)^2,$$

or, "Every cube is the difference of two squares, one at least of which is divisible by 9."

(*b*.) If

$$x^3 + y^3 = z^3,$$

then

$$(x^3 + z^3)^3 y^3 + (x^3 - y^3)^3 z^3 = (z^3 + y^3)^3 x^3.$$

This furnishes an easy proof of the impossibility of finding two integers the sum of whose cubes is a cube.

Monday, 4th April 1870.

The Hon. Lord NEAVES, Vice-President, in the Chair.

At the request of the Council Professor Wyville Thomson, Belfast, delivered an address on "The Condition of the Depths of the Sea."

Monday, 18*th April* 1870.

Professor KELLAND, Vice-President, in the Chair.

The following Communications were read:—

1. Facts as to Brain-Work; in Illustration of the New and Old Methods of Philosophical Inquiry in Scotland. By Thomas Laycock, M.D.

A few words in explanation are needed. In my summer course of lectures on Medical Psychology and Mental Diseases delivered in the University, I have to investigate the human mind in its practical relations to the body, and especially I have to teach how each influences the other, so that the physician, or any intelligent person, may be able to modify these relations beneficially. The starting-point in these inquiries is the fundamental fact of experience, that no changes in the mind or the consciousness of whatever kind can or do arise, or continue, without a corresponding series of changes somewhere in the brain-tissue. This fact being held as certain as the fact of gravitation, the solutions of the problems to be solved depend upon a knowledge of the relations which the two series of phenomena bear to each other; for which knowledge it is necessary to analyse and classify the varying states of consciousness on the one hand, and the changes in the brain-tissue which correlate them on the other. As to the last mentioned, it is certain that they are vital; they come, therefore, under the sciences of Life collectively termed biology.

But all molecular changes in living tissues, of whatever kind they may be, and consequently those of the brain, can be brought also within the circle of molecular physics, for they can all be resolved into motion of something, whether we designate that something an atom, a molecule, a vortex, a ring, or a centre of force. They are due, therefore, to energy; or, as distinct from mind, to motor energy. The Rev. Professor Haughton, M.D., of Dublin University, was led by experimental research to the conclusion, that as much motor energy is expended in brain-work in five hours as in muscle-work—say by a street-paviour—in ten hours. Although all the changes going on in living tissues may be finally resolved into chemical changes,—a fact well illustrated by Dr W. B. Richardson, and by Professor Crum Brown's and Dr Thomas R. Fraser's valuable researches into the connection between physiological action and chemical composition, lately communicated to the Society,—they are distinct from those induced in inorganic matter by chemical affinity, and hence the need of connoting the energy by the term vital. Now the distinguishing character of that energy, whether manifested in plants or in animals, is adaptation of all motion to ends. Evolved in the brain, this vital energy is manifested as mind, and life is thus spiritualised. I would even venture to say that matter is thus immaterialised, for since all states of consciousness correlate motion of something, it is not the connection of mind with mere ponderable or brute matter we have to discuss, but of mind with adapted motions in infinite variety. All external impressions received through the senses and exciting states of consciousness can be resolved into motions that can be exactly measured, in regard to impressions on the eye and ear, and all internal impressions passing from one part of the brain or of the body to another part, can be resolved also into an energy correlative with motion, termed *vis nervosa*. So that psychology by this method is, in one sense, a department of physics; in a wider sense it is a science or philosophy of nature, and therefore differs essentially from modern physiology, which is only a restricted department of physiology in the true and ancient sense of the word. In fact, the method I adopt is an adaptation of the ancient Aristotelian method to modern philosophy, and in adopting it with me, the Faculty of Arts of the University would only return to a former arrangement of work. Sir William Hamilton observes on this point to the effect, that "Aristotle's treatise *On the Soul* being (along with his lesser treatises on *Memory and Reminiscence*, on *Sense and its Objects*, &c.) included in the *Parva Naturalia*, and he having declared that the consideration of the soul was part of the philosophy of nature, the science of mind was always treated along with physics."*

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* Lectures on Metaphysics, vol. i. p. 127.

The cause of this change in Faculty-work was, in fact, the rise of different methods of philosophical inquiry named the reflective, which discarded all observation and experimental research whatever. Sir William Hamilton explicitly taught that the only external condition needed for philosophical inquiry is a language "capable of embodying the abstractions of philosophy without figurative ambiguity,"—a condition not yet attained, however, nor likely to be. "With this one condition," Sir William declares, "all is given; the philosopher requires for his discoveries no preliminary preparations, no apparatus of instruments and materials . . . . it is only necessary that the observer enter into his inner self [and here is truly a figurative ambiguity of language] to find there all he stands in need of."* Hence the reading and writing of books, and discussions of opinions, are the proper results of reflective inquiry. It was to his extreme devotion to the literature of philosophy that was due that lamentable palsy of the sign-making organs, the right hand and speech-muscles, termed *aphasia*, with which he was afflicted, for these were overworked in the acquisition of that immense erudition which distinguished him. The locality of the brain-disorder in these cases is in the anterior lobes, more especially the posterior third of inferior frontal convolution.

Although the principles of the reflective method there laid down by its greatest modern master exclude observation and experimental research, Sir William Hamilton did not neglect physiological inquiry. My own researches into the reflex and unconscious functions of the brain, made twenty-five years ago, were rewarded by his highly valued approval and friendship, because he saw in them the physiological side of his doctrine of "latent" consciousness; but the kind of inquiry he followed was physiological in the restricted sense of a physiology of the human brain, and not in the wider sense of a science of nature. But I do not advocate this restricted method as the best or even a true method of philosophical inquiry, nor do I wish to defend the errors to which it leads. I speak only for my own method as just explained.

Matters being thus, it interested me to read the manifesto of principles and methods which my reverend and respected colleague, the Professor of Moral Philosophy, gave forth when he took possession of his chair in November 1868, and which he published under the title of "Moral Philosophy as a Science and a Discipline." In this essay he specially criticised the physiological method, and in such a way that the Professor of Physiology thought it expedient to publicly controvert his views. The facts I have to place before the Society having a reference to this criticism, I quote it. Professor Calderwood said, "There are evidences of great activity on the part of upholders of a sensational philosophy, differing only in its modifications from that which Scotland formerly rejected under the leadership of Reid and Stewart. In conjunction with this revival of sensationalism, there is eagerness not only to combine physiological and mental science, but even to question the sufficiency of our investigations regarding the facts of consciousness—to make nerves and muscles the only safe approach to a science of mind,—and to proclaim the necessity of making physiology the basis of psychology. The consequence of this is, not only that mental philosophy is being encumbered with irrelevant investigations concerning such physical processes as mastication and respiration, and such physical experiences as toothache and cramp in the stomach, but we are involved in all the hazard connected with the use of a false method." I gather from this sentence that my reverend colleague, however opposed or misinformed he may be as to the physiological method, certainly means not only to defend and resolutely maintain the sufficiency of the reflective method as laid down by his great master, but to assert its superiority over the Aristotelian method of observation and research. Now, it is upon these points that I join issue with him. I shall select two problems for illustration, taken from my respected colleague's own department, viz., the nature of belief and of personal identity, being guided to the selection by his own declaration, viz., "The supposition that physiology can lead us to philosophy of mind, is doomed to rejection by all to whom it is clear that our personality is not essentially connected with our body, which is only a temporary dwelling," &c. In this condemnation of physiology is included the assertion of the psychological proposition that mind, considered as an energy or principle, is separable from life, and that it only occupies the living body as a temporary tenant. Now, the holders of this opinion have, in common with the physiologists, a belief in a future life, and follow two methods of inquiry as to that truth of religion, viz., the *confirmatio veri* and the *inquisitio veri.* The spiritualists (so-called) have adopted the latter or scientific method, the orthodox philosophers the former. To this end they state certain propositions as unquestionable. Firstly, that every man assuredly believes he is a mental unity, one, or Ego; secondly, that "our thinking Ego . . . is essentially the same thing at every period of its existence,"—I quote Sir William Hamilton, vol. i. p. 374; and, thirdly, that the evidence upon which these assumed beliefs are founded is sufficient, being that of consciousness itself. In other words, I feel assured that I am one and the same person that I ever was, and therefore I am one and the same. Is this evidence sufficient? Can we rely absolutely and without need of verification upon the veracity of consciousness manifested as belief? To answer this question clearly, it is necessary to understand how beliefs arise and are modified. Now, since according to the fundamental fact that every state of consciousness coincides with corresponding molecular change in brain-tissue, we conclude that all beliefs, being states of consciousness, must be coincident with such changes. Is this conclusion true in fact? First, as to the Ego. A man, like other mammals, is one in body—a corporeal unity—in accordance with the fundamental biological law of organisation *ad hoc.* The belief that he is one, or Ego, bodily, is founded upon his knowledge of this fact. The belief that he is a mental unity, or a thinking Ego, correlates, as I shall shortly show, the unity of cerebral function manifested in the various states of consciousness of the man at any given moment. But the belief that this Ego, whether corporeal or mental, is essentially the same thing at every successive period of a man's existence, includes wholly different phenomena, since it refers to past time, and consequently implies a reminiscence of what it was at some moment of past time, or in past time generally. Now, reminiscence is proveably dependent upon a recording vital process, whereby we are enabled to know in time present by virtue of the so-called association of ideas—what we were, and thought and did in past time. If there be no record or memory, or if there be a record, but no association of ideas so as to induce reminiscence, then there is no knowledge of past mental states. What is essential, therefore, to belief in continuous personal identity as a mental state, is that consecutive continuity of vital processes which is necessary to reminiscence, and not a continuous consciousness, as is the doctrine of reflective philosophy. Memory in this sense may, and does extend in fact beyond the consciousness, so that changes may and do take place in the consciousness which are due to preceding records made without consciousness, but which not being for that reason recognised as belonging to past mental life, are believed to be intuitive. Memory in the individual from this point of view, and considered as a vital process, has its exact counterpart in what may be termed memory of the species of both plants and animals, in virtue of which consecutive continuity of vital process through the seed or germ is maintained, and ancestral qualities reproduced in offspring.

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* Lectures on Metaphysics, vol. i. p. 383.

Such being the philosophy of belief, considered as the result of brain-work, it is not difficult to understand why the philosophy of morals, in so far as it is founded on identity of belief simply, or orthodoxy, and not upon knowledge, is chaotic ; nor how it is that all the efforts made to secure identity of mere belief, independently of knowledge of the order of nature, whether by education or otherwise, must fail.

I shall now illustrate these views by morbid or insane beliefs. The reflective philosophy, as is well-known, discards all inquiry into aberrant mental states ; with much the same propriety, however, as an astronomer would discard the observation of planetary observation : in the inductive method these are of the greatest value as experiments of nature. By examining every kind of result of the molecular change as manifested by others, and comparing these with our own, we are enabled in truth to study them as directly manifested to our own consciousness. Hence all facta, all writing, all art, and all conduct, however normal or abnormal, are the appropriate facts for inductive inquiry. To illustrate the method in this direction, and at the same time to show the true relations of belief, I place before the Society the portrait of a house-carpenter painted by himself, with a descriptive legend describing himself as three persons, viz.—1. George Elliot, his true personality. 2. " George the Fifth, son of George the Fourth ;" and, 3. " The Emperor of the world—the true and lawful God." The reflective philosopher would think it a sufficient explanation to say that the man is a lunatic. He should remember, however, that he owes this explanation to the physiological method. Formerly, the explanation, according to the reflective method was, and with many still is, that the lunatic is either inspired or else possessed by a spiritual being. The inductive philosophy, starting from the fundamental fact that all states of consciousness of a man, however manifested, cannot be manifested independently of vital processes, lays down the law that in the living man Life and Mind are inseparable, and consequently that the "thinking Ego" is the man himself. Now, although his person is double, whether as to limbs or brains, his corporeal condition of unity is no more affected thereby in a healthy state than the unity revealed in consciousness—the one being the reflex of the other. His two brains act together so as to attain the unity of consciousness, just as his two eyes act in unity of vision; but as he may see double when the two eyes act disjointly, so may he have a double consciousness when the two brains act disjointly. Whether he believes, or whether he doubts that he sees two objects, or that he is one or two persons, depends upon those molecular conditions upon which the belief and doubt of the moment depend. Or, again, just as an object of vision may, from disorder of the corresponding brain-tissue, appear to a man to be something wholly different, as when his friend appears to be the devil, constituting what is termed a hallucination, so his personality, from disorder of the corresponding brain-tissue, may appear to be something wholly different, and he may chance to have an hallucination that he is the devil. It appears probable, therefore, that although a man may have many and various delusions as to his state of mind and body, he will rarely exceed three distinct and fixed delusions as to his personality, viz., one resulting from disorder of each brain acting disjointly, and one from disorder of both acting conjointly. Under the restrictions stated, the result of numerous observations I have made is in accordance with this view. So much for the break-up of the unity of consciousness by brain disorder. It is obvious at a glance that these diversities of belief as to personal identity are associated with brain changes involving memory and reminiscence; otherwise, when Elliot came to a belief in his royal birth and parentage, he would also remember, to the confusion of the belief, that he is and always has been George Elliot the house-carpenter; or, at least, a reminiscence, however vague, would induce doubt. But no such results followed, and the belief is fixed and unwavering.

These considerations apply to belief only; but to understand the questions at issue better, I shall inquire how a man comes to doubt, and what is essential to as accurate knowledge as he can attain under the circumstances. For this purpose I shall select the state of consciousness known as dreaming. No well-informed inquirer now holds the doctrine that in that state man is inspired, or that the soul or mind acts independently of the body; it is admitted that every such change of consciousness as constitutes dreaming is directly dependent upon molecular changes in the brain-tissue. In accordance with the physiological law already laid down, the dreamer believes in the reality of his dreams, however absurd they may be, and however far removed from the normal conditions the molecular changes. It is only when he awakes, and the normal condition is restored, that he doubts or disbelieves. Now, an analysis of these purely physiological phenomena shows that those states of consciousness which in the waking condition of the brain are either reminiscences or anticipations, have in dreams no true element of time, either past or to come; they are either wholly of the present, or have no true relation either to time or to space. Memory, therefore, as the knowing reminiscence of past states of existence, and judgment as the perception of the future, are abolished. Memory of the past is abolished, on the one hand, because the association of ideas upon which that faculty depends, and which began at some past time, is abolished; while, on the other hand, there is no knowledge of any existing personal relations to time and space, because the senses being shut, there is no perception possible of these relations. Hence the merest phantasms of the imagination, admittedly due to molecular changes induced under these conditions, are received as verities. Reid relates how, on a certain occasion, when he slept with a blister on his head, he believed he was being scalped by Indians. It is only on awaking, when memory, and external perception, and normal associations of ideas are restored, that a true knowledge of the fallacious character of the beliefs can be attained. Hence it is clear that these conditions are necessary to a right belief in continuous personal identity. These conclusions are strictly applicable to all hallucinations and beliefs of morbid origin. Many persons have delusive beliefs during the waking state as transient as dreams. This is very common in the brain-failure of old age. Delusive beliefs, more strictly insane, may come and go in like manner in the earlier stages of an insanity. I had a patient under my care, in whom they came on only when he was in a heated room, and who could recover from them by the cold douche applied to the face. In cases like George Elliot, the morbid state is best described as a fixed dream. When those molecular changes, which coincide with the mnemonical records of his daily life, of things done, succeed each other, he truly believes he is George Elliot, a house-carpenter; but when the mnemonical records of his dream-life, and which are wholly dissociated from the former, are presented to the consciousness, then the associated personality is presented also, and, for the time being, he believes as firmly he is another person than George Elliot. These delusive states may have every degree of duration. In certain kinds of waking somnambulism, the individual lives an actual life, as two wholly dissociated personalities, for hours or days alternately, the mnemonical records of the two being quite as dissociated as dreaming and waking life; or they may occupy only a few moments, as in the artificial somnambulism induced mesmerically, where the brain has been so acted on that the patient is made to hold the most absurd beliefs,—to believe, in short, whatever he is told is real. In this way Sir J. Young Simpson changed the personal identity of two ladies in regard to the husband of one of them, so that the unmarried believed she was the married, and *vice versa*. From these facts, and they might be multiplied to any extent, it is clear that the notion or belief of personal identity is not due to mind in the abstract, considered as an immaterial substance acting in entire independence of life and organisation, but to mind in the concrete, as inseparably associated, not with brute inert matter, but with the motions and forces upon which life depends. This, I need hardly say, is no new doctrine of philosophy, whether profane or biblical. The earliest record of Scripture affirms that man only became a living soul after the breath of life was breathed into his nostrils; and St Paul, the philosophic apostle, adopting this view to explain the resurrection, uses the biological analogy of the continuous life of the species of plants through the germ, to indicate how the individual or personal life of man may be continued independently of consciousness, and how it may be evolved into consciousness at some future time, plainly adopting thereby the Aristotelian doctrine of the soul.

Many attempts have been made to verify the separate existence of the soul, whether as a religious dogma or a philosophical doctrine, and, of necessity, all have failed. I have placed before the Society an illustration of these attempts, by the so-called spiritualists, to prove the fact of an independent personal identity. It is a drawing, by a member of an eminent literary family, of the spirit-emblem of a distinguished and much esteemed fellow of this Society. Here are published representations of like emblems, taken from Mrs Newton Crossland's "Light in the Valley." The seeress, we are told, who beholds these mystical appearances, describes them as appearing to her in colours of liquid light, with the utmost clearness, more rich and radiant than earthly jewels. These emblems are usually seen to be situate behind the persons to whom they belong, the centre of the emblem rising just above the head, and occupying a circumference of several feet. They are the badges by which persons are recognised in the spirit-world, even while they remain on earth. To the production of these emblems a belief in the separate existence of "spirits" is essential—doubt, like the waking from a dream, either prevents or dispels the phantasies. Physiologically they differ in no respect from the delusions of George Elliot, or of dreamers. The verification of any belief means the investigation of the order of nature, so as to determine whether the conclusions presented to the consciousness as brain-work coincide with the natural order of events. To those who are confident that they can assuredly believe in their own eyes, the sun undoubtedly moves, and the observer is motionless, but a verification of the conclusion shows that the motion is in the observer, and the sun is motionless. Now, when a spiritualist attempts to verify his belief in spirits, he ignores the fact that his belief is due to molecular changes out of, at least, direct relation to any spiritual influence, except that which constitutes his own spiritual nature, and is thus led to esteem the mere phantasms of his own imagination as proof of external agencies which may exist, but which, by the terms of the hypothesis, cannot be verified. Resolved into their ultimate elements, all the so-called proofs of spirit-life, when stated *bona fide*, are simply presentations to the consciousness of the inquirer's own brain-work, as delusive as those of the lunatic or the dreamer. It has been commonly said that this class of inquirers are, for the most part, either of weak mind, or credulous, or ignorant. But this is not so. Here are delineations of the od-force, as investigated by Baron von Reichenbach, a skilled scientific inquirer. He never saw what is here represented as the manifestations of the od-force, he simply shows what was described to him as such by hysterical and morbidly nervous women; and if they be true as descriptions, they are only representations to the consciousness of phantasmal brain-work. Some of these so-called spirit operations are instructive illustrations of æsthetical automatic action of a cultivated brain. The emblem of a fellow of this Society, drawn by a person of high culture, is contrasted well with the uncouth mystical emblems of an uneducated female lunatic before me. I was assured by my late friend David Ramsay Hay, and no one was more competent to judge, that it is exactly true to the geometrical principles of form and colour.

In the delusions of George Elliot we have an illustration of another interesting result of brain-work, the ideational evolution of the intuition of the infinite, a subject so much and so earnestly discussed by reflective philosophers, and which is equally as capable of biological illustration as the preceding.

2. On Change of Apparent Colour by Obliquity of Vision.

By Robert H. Bow, C.E., F.R.S.E.

I discovered the peculiarity of chromatic vision, which is the subject of this paper, in the month of January, when conducting some experiments upon the perfection of definition at different parts of the retina; and I may introduce the subject by first referring to these experiments.

In the case of ordinary sensation seated in the skin, there are two offices performed by the nerves—first, that of informing the mind of the fact of the contact or impression being made; and, second, that of giving more or less minute information as to the locality of the sensation. Professor Weber experimented upon the latter power, by testing the least distance apart at which two objects touching the skin of any part of the body could be felt as two distinct sensations; and, as you are aware, this tactile power bears no constant proportion to the mere power of feeling a sensation of contact. For instance, the back of the hand is perhaps more sensitive to a simple contact than the tip of the finger, but Weber found that the points of contact are required to be fourteen times further apart at the back of the hand than at the tip of the finger, before they can be distinguished as separated.

Now, a very strong analogy exists between these two functions of ordinary sensation and corresponding offices of the retina. Objects seen obliquely are not strikingly different in brightness from the same seen in the direction of the optical axis, but the power of definition (apart altogether from mere optical causes) varies immensely. I attempted to investigate this defining power for different parts of the retina by a method exactly analogous to Weber's—namely, by inspecting two white spots on a blackened card, and determining, for different angles of obliquity and direction, the greatest distance from the eye at which these spots could be detected to be double. But I soon found that, when the vision is very oblique, there is a puzzling feeling of uncertainty as to the result; and it occurred to me to assist the judgment by substituting for the white spots objects of contrasting colours.

On attempting to put this idea into practice, I made the important discovery, that when coloured objects are inspected under oblique vision, the colours are at the same time reduced in intensity, and changed in character: thus, *scarlet* becomes successively orange, yellow, and whitish-yellow, according to the obliquity; *green*, of a medium character, tends to become white, and *violet* to become blue.

In experimenting upon the subject, it is best to place the coloured object obliquely on the nasal side of one eye, the other eye being closed; much smaller angles of obliquity bring about the phenomena when seen on this side of the eye, and we get rid of any complicity with the insensitive spot where the optic nerve joins the retina. I may point out here, however, an experiment that shows the general peculiarity, and also the excess of change that takes place when the object is on the nasal side compared with the other. Against a dark-coloured wall hold up, at arm's-length, an orange-coloured object of three or four inches in diameter. We suppose it held by the right hand; then turning the face rather towards it, look at a point in the wall eighteen or twenty inches to the left of the object; and now closing the eyes alternately, it will be observed that, when the right eye is open, the object will appear of nearly its full orange colour, but when the right eye is closed and the left opened, the object will assume a pale, sickly, yellow tint; and if the point in the wall be taken further from the object, the colour seen by the left eye will approach nearer to white. To cause the same amount of change to the right eye, the obliquity must be very much greater. Another mode of conducting the experiment, as depending upon the contrast of effect upon the two sides of the eye, is this: Choose two objects of the same colour, place these two or three inches above or below a mark on the wall, close one eye, and with the hands withdraw the objects equally away on either side from the central position, the eye being rivetted to the mark on the wall; it will then be noticed that, relatively, the object on the nasal side of the observing eye undergoes a rapid change of tint or colour. But, it may be repeated, the most satisfactory mode of examining the changes is to use one eye and observe with the coloured object on the nasal side of it, the eye being held steadily upon a mark, which may or may not be of the same colour as the object. Observed in this way, the following changes will be presented:—

First. The colours lose more or less their chromatic intensity, and approach nearer to white or black, according as they are placed upon a dark or light ground. But extreme red is especially marked as losing illuminative power, as well as chromatic character. Ultramarine blue, on the contrary, appears to lose very little by oblique vision; it assumes a lighter blue hue.

Second. The colours undergo a change of chromatic character.

a. Brilliant scarlet, painted with biniodide of mercury and gum arabic.—This, when placed on a dark ground, and observed at an obliquity of about 30° on the nasal side, appears orange; at 40° to 50° it looks of a somewhat meagre yellow, beyond this a pale yellow. As seen at the outside of the eye, the orange only appears when the obliquity reaches 80°, and the yellow at 90°.

b. Some *orange* colours show the change very markedly to yellow, and to nearly white.

c. *Emerald green.*—This, at 40°, becomes nearly white, generally yellowish.

d. *Ultramarine.*—This is very persistent, visible at 40° as a blue.

e. *Pink*, of a purplish cast.—This in day-light, when placed on a white ground, appears—even at a very moderate obliquity—a purplish *blue;* if placed on a black ground, it assumes a lavender *blue* colour.

This change of purples and pinks to blue is one of the most striking; perhaps the best way of witnessing it is to use two thicknesses of cobalt blue glass, fortified with a pink or purple one, so as to allow both extremities of the spectrum to pass freely. This screen, held before a gas light, appears by direct vision of a fine pink colour, but by a moderate obliquity it is reduced to a bright blue.

f. A bluish-*green* glass, held in front of a gas light, appears to become blue by oblique vision.

g. A yellowish-*green* glass becomes by oblique vision more decidedly yellow.

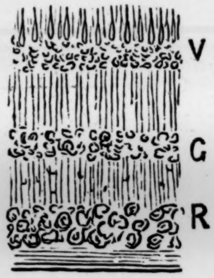
Remarks and Speculations on the Phenomena.

Under oblique vision the purples or pinks become blue, and the extreme red becomes dull. It would appear, therefore, that towards the margins of the retina the sensation of *blue* is less reduced in intensity than that of *red*, and a step in the explanation of the results is this: the red in the purple or pink becomes a dull orange or yellow under oblique vision; this gives rise to the sensation of white light when combined with a part of the blue, and reduces the remaining part of the blue to a paler cast. The same explanation applies to a blue-green becoming blue—the green becomes white or pale yellow under oblique vision, and so dilutes the blue ingredient to a paler shade.

The second observation that may be made upon the results is, that by oblique vision the various colours are seen under the same conditions as in the most common form of colour-blindness, wherein there are really only two colour-sensations, the upper half of the spectrum, from blue-green up to violet, and including pinks and purples, appearing *blue;* and the lower half, from yellow-green down through yellow, orange, and scarlet, to bright red, appearing *yellow;* and in such colour-blindness the extreme red is frequently very dull. We may, therefore, expect the discovery of some similarity in the conditions of the central part of the retina of an eye affected with this form of colour-blindness, and the marginal parts of the retina of a normal eye.

Before concluding, I would venture to connect the discovery with an existing theory of colour-sensation, as it may help to establish that theory, should a prediction the connection leads to be found to be correct.

The figure here given shows a section of part of the retina (Kölliker). Now, it has been suggested that each of the layers V, G, and R, is receptive of the sensation of light,—the layer V being affected by the more refrangible rays blue and violet, R being affected by the less refrangible yellow, orange, and red, while the central layer G is affected by the central parts of the spectrum, blue, green, yellow, and orange; and this would account for the approximate achromaticity of the eye, for when the eye is arranged for the most acute vision, the focus of blue rays will correspond with V, of green rays with G, and of scarlet rays with R.



But it is well known that the eye does not see any colour quite purely; there is always white light present, or, in other words, one of the layers, V, G, or R, cannot be agitated or excited without the others partaking to some extent in the excitation. Now, there is a probability that the degree of freedom with which one layer may transmit its special sensation without one or both of the others participating, to an important degree, in the excitement, depends in part upon the maintenance of a considerable interval between the layers. Let us then imagine the interval between G and R to become more or less perfectly obliterated, and it is evident that no simple sensation of red or green could be felt, but only a colour-sensation, which corresponds with the excitement of both of these layers, which is *yellow*. It may, therefore, be worth the attention of anatomists, skilled in working with the microscope, to ascertain if any decided reduction of the interval G to R takes place towards the margins of the normal retina, or has place in the central part in eyes that have shown, during life, the commonest form of defective vision of colour; we should also expect a reduction of the interval V to G, but to a less decided degree. In the case of an eye completely colour-blind, we should look for the coalescence of the three layers into one, unless the defect were accounted for by the absence or paralysis of two of the layers.

The following motion by Mr Sang was considered:—

1. Every Communication intended for the Society shall be submitted to the Council, and passed by them as not containing anything objectionable, before being mentioned in the Billet.
2. The Society shall not take up any matter which has not been announced in the Programme, except in cases of extreme urgency.

The motion was not adopted, as the Society thought that Mr Sang's views were already embraced in the printed regulations for the order of business.

Monday, 2d May 1870.

DAVID MILNE HOME, Esq., Vice-President, in the Chair.

The following Communications were read:—

1. Remarks on the Theories of Capillary Action. By Edward Sang, Esq., F.R.S.E.

That theory of capillary action, which seems to have satisfied the greater number of physicists, is founded on the assumption that the particles of a fluid are separated by distances immensely great in comparison with their magnitudes, and that these particles attract each other,—the sphere, however, of their attraction extending to a distance infinitesimally small in comparison with the observed disturbances of the fluid-level.

The accommodation of this theory to the actual phenomena is accomplished by long operations, comprehensible only by those who are familiar with the higher calculus. The object of the present paper is to examine this theory in the light afforded by a general knowledge of the leading laws of mechanical science. For this purpose, the author proceeds to analyse the ordinary phenomena of the rise of water round a piece of clean glass which has been plunged into it. Assuming a fluid particle situated upon the inclined surface, he observes that, according to the hypothesis of an infinitesimally small sphere of attraction, this particle is beyond the direct influence of the glass; the only other influences to which it is subjected are gravitation and the attraction by the adjacent fluid particles.

Now, according to this same hypothesis, the particle is attracted by that part of the fluid which is within a small sphere described around it; but the curved surface, having its radius of curvature infinitely greater than the radius of this sphere, may be regarded as *flat* within the range of attraction, and therefore the solicitation, to which the particle is exposed, must be exerted in a direction normal to the surface. By a more minute examination, the author shows that, if the radius of the sphere of attraction be reckoned as a differential of the first order, any deviation from normality must belong to the third order of differentials—that is, must be of an order infinitesimally smaller than the infinitesimally small sphere of attraction.

Thus the only two solicitations to which the particle can be subjected are, the attraction of the fluid exerted in a direction normal to the surface, and gravitation. Now, it is impossible that the resultant of these two solicitations can be normal to the surface; but no fluid can be in repose if the attraction exerted upon a particle at its surface be not normal to that surface, wherefore, the author of the paper concludes, the infinitesimally-small-sphere-of-attraction-hypothesis is untenable.

On considering the hypothesis of attraction generally, the author proceeded ro remark that, in order to prevent the condensation which would result, we must suppose some resistance to the farther approach of the particles, which we may typify by a repulsion; and that these tendencies—the attractive and the repulsive—must be in equilibrium. A theory, then, which takes into account only one of these equilibrated antagonists, must necessarily be defective. And since, in all cases, the attraction supposed to exist between two sets of particles must necessarily be resisted by actions between them, there can be no tension like that which has been supposed to be exhibited by the superficial films of fluids.

2. Theory of Construction of the Great Pyramid. By John Christie, Esq. Communicated by the Rev. W. Lindsay Alexander, D.D.

In his early investigations on the principles of construction of the Great Pyramid, the author was forcibly struck with the following fact—viz., that if a perpendicular be drawn through the apex of the Pyramid to its base, and the unit angle with the horizontal thrown up from the base on each side of this perpendicular, the angle comes out on the faces of the Pyramid at the openings of the north and south ventilating air-channels; at the same time he was led to the conclusion that one-tenth of the base line, and the same tenths also applied to the faces of the Pyramid, ruled the entire structure. Following this out, and having erected perpendiculars on each of these tenths, and horizontals from each of the facial divisions, the first step procured a grand central point—viz., in the centre of the grand gallery; the next step was to account for the position of the King's Chamber, by the intersections of the first and second circles—used in the construction of the Pyramid, as shown in Diagram No. 1. Having thus obtained a central perpendicular for the King's Chamber, he then made use of the direction of the celestial equator, and where it cut the last-named perpendicular, a third point was gained as a centre for the third circle, which completes the Pyramid in its external form. He next found, that by connecting the south outcrop of the air-channel with the north corner of the base, a parallel was gained for the angle of the grand gallery. By drawing a horizontal line between the two air-channel mouths, and dropping perpendiculars from these to the base, two oblongs are formed, one on each side of the axis; the diagonals of each of these being the unit angle.

The astronomical bearing of the Pyramid seems manifestly to be indicated in the sections of the King's Chamber. In the section of it in its breadth, the chamber is filled up by—first, a section of the Pyramid itself, the base of which is the floor line of the chamber; the space above, as regards height, being filled by an equilateral triangle, its angles 60°, corresponding as they do with the direction of the celestial equator, 60° seem to point with threefold force to the fact that the Pyramid has a direct reference to the sun.

The same is twice repeated in the section of the King's Chamber in its length, the length of the chamber being exactly twice its breadth. Another very marked reference of the same kind occurs in the position that the Queen's Chamber bears to the King's Chamber. If an equilateral triangle, whose apex is in the centre of the floor of the King's Chamber, be constructed, having its base in the base line of the Pyramid, the centre of the floor of the Queen's Chamber will be found to be exactly in the middle of the north limb of this triangle, other instances are also shown to be regulated by the equilateral triangle.

The unit angle regulates the length and height of the King's Chamber, the space between it and the ante-chamber, the form of the ante-chamber, and the distance to the great step, also the interior length, breadth, and depth of the much-abused granite coffer.

Coffer Unit Block.

Breadth, $\frac{1}{18}$ part of interior length of coffer.
Height, $\frac{1}{5}$ „ „ depth „
Thickness, $\frac{1}{5}$ „ „ breadth „
90 of these cover one side of coffer.
90 „ bottom „
450 exactly fill coffer.

The shape of this block is regulated by the unit angle in its top sides and face, and consequently conserves the Pyramid facial angle, which it would not do had its length, breadth, or thickness been different, in which case the complement of these blocks would have been too large or too small for the coffer content.

Record of Physical Facts—Water-Levels.

The King's Chamber is a noted index of these. That this was intended by the Pyramid builder seems to be demonstrated by the fact of the rock on which the Pyramid stands having been scarped down to the level of the Pyramid's base, so as to procure a horizontal line midway between the external physical fact to be recorded, and the internal index of that fact contained in the King's Chamber, serving as it did, at the same time, astronomical purposes, neither of which would have held good had the rock not been so scarped down.

These water-levels have been previously indicated by other modes than those by which they are now illustrated. It will be observed that the circles used to indicate them have also peculiar references to other parts of the Pyramid besides those they bear to the King's Chamber. One marked instance may be noted here. The circle, which indicates the High Nile-level, touches the floor of the King's Chamber in the centre, and also indicates the angle of the floor of the grand gallery. Reference may also be made here to one of the circles used in the construction of the chambers and passages, it being of a very marked and significant character. This circle has its centre in the Pyramid's base, in the point where the "direction of the celestial equator" cuts the base, its radius is the prime central point in the centre of the grand gallery, and in its course it touches—1*st*, The mouth of the entrance passage; 2*d*, The step leading down to the Queen's Chamber; 3*d*, The "bottom of well" in the lower part of descending passage; 4*th*, Bounds the Low Nile-level; and 5*th*, Where it cuts the lower portion of the direction of the celestial equator, the High Nile-level. The difference between the mean Nile-level and the mean sea-level is indicated by an equilateral triangle, the apex of which is in the mean sea-level, and the base the mean Nile-level, the length of the latter being contained between two perpendiculars—the first from the north corner of the Pyramid's base, the second from the first remarkable perpendicular joint in the entrance passage.

Independent Methods of Constructing the Great Pyramid Externally.

1*st.* Give a horizontal line. Bisect it, erect perpendiculars at both ends and also from the centre, from one of the ends throw up the unit angle with the vertical, and through the point where the angle cuts the opposite perpendicular draw a horizontal line, an oblong will thus be formed, the diagonal of which is the unit angle, join the top of the central perpendicular with the lower corners of the oblong, and the Pyramid is complete.

2*d.* Given a vertical line, the radius of a circle, at right angles, through the centre of circle, draw a horizontal line, bisect the vertical line, and throw down the unit angle with the vertical from both sides of the vertical at its bisection, through the points where these cut the horizontal line, join the extreme end of the radius, and the Pyramid is complete.

The Diagrams submitted to the Society were as follow:—

Diagram No. 1.—Construction of the Great Pyramid in its external angles, its chambers and passages by the unit angle, and one-tenth of the base, on a given horizontal line.

Diagram No. 2, one-sixteenth of the full size.—Sections of the King's Chamber, in its length, and also in its breadth, showing how it is regulated by the unit angle, &c.

Diagram No. 3, one-half of the full size.—Sections of the granite coffer in its length, and also in its breadth, showing how it is regulated by the unit angle and conserves the Pyramid facial angle.

Coffer unit block, in further illustration of Diagram No. 3.

Diagram No. 4, one-sixteenth of the full size.—Section of the King's Chamber in its breadth, the ante-chamber, great step, and south end of grand gallery, showing that the space between the King's Chamber and ante-chamber, the form of the ante-chamber itself, and the distance to the great step, are all regulated by the unit angle; showing also the references between a portion of the chambers of construction and the overlappings of the grand gallery.

Diagram No. 5.—Independent method of constructing the Great Pyramid in its external angles from a unit angle oblong.

Diagram No. 6.—John Taylor's theory of the reference the Great Pyramid bears to the circle, with Professor C. Piazzi Smyth's amplification of the same, and further amplification by the author.

3. On the Structure of Tubifex. By W. C. M'Intosh, M.D.

The paper consisted of a detailed account of the external form; the arrangement of the body-cavity and its walls; the perivisceral space and corpuscles; the digestive, circulatory, and generative systems.

It was specially mentioned, in regard to the perivisceral corpuscles, that the author was not at all inclined to think that they originated from the glandular fatty coating of the digestive tract and the dorsal blood-vessel. The corpuscles seem rather to be the product of the perivisceral cavity itself and its special (free) contents. This view requires no stretch of ordinary physiological principles, and is quite in keeping with what is found in other groups. In the Nemerteans, for instance, a complex corpusculated fluid is produced within a closed chamber with smooth walls.

The following Gentlemen were elected Fellows of the Society:—

James Sime, Esq.
Thomas Harvey, LL.D.
John Young Buchanan, M.A.
John Hunter, M.A., Belfast.
The Right Hon. The Lord Justice-Clerk.
The Hon. Lord Gifford.

Monday, 16th May 1870.

Dr CHRISTISON, President, in the Chair.

On taking the chair the President alluded to the loss which the Society had sustained by the death of Sir James Y. Simpson, Bart.

The following Communications were read:—

1. Primitive Affinity between the Classical and the Low German Languages. By the Hon. Lord Neaves.

(*Abstract.*)

In this paper the author adverted to the limited attention that was paid in this country to comparative philology, and noticed the principles it had developed and the progress it had made elsewhere of late years.

In illustration of the results thus attained in the Aryan or Indo-Germanic languages, he took as familiar examples the affinities that could be traced between the Latin and the Old English tongues, viewing the Latin as a type of the earlier branches of the family, including the Greek and Indian; and the English as a type of a later branch, consisting chiefly of the Low German dialects. The affinities referred to were not those which connected Latin with English through the romance languages, but those which subsisted between Latin and vernacular English, and which must have arisen from a prehistoric identity or connection.

The chief law regulating these affinities was what is commonly called Grimm's law, but which is subject to various limitations and exceptions.

The affinities between words in cognate languages which have had no historic connection are to be found out—1st, by studying the general law of letter-change prevailing between the primary and secondary branches of the family; and 2d, by finding out the peculiarities or idiosyncrasies of the individual languages sought to be compared; for each language has a character of its own, and both Latin and English have strong peculiarities distinguishing them from other languages, which help to conceal cognate words from each other, and which must be mastered before the double disguise can be seen through.

He exemplified these views by detailed instances, and concluded by urging that all nations of the Aryan race ought to be regarded as susceptible of the highest culture, and that the good hopes might be entertained of their being all raised to as elevated a state of Christian civilisation as the best of them had attained.

2. On the Genetic Succession of Zooids in the Hydroida. By Professor Allman.

In this communication an attempt was made to express by means of formulæ the various modifications presented by the life series of the *Hydroida*. It was also shown that there existed among the *Hydroida* both centripetal and centrifugal forms of development. These were compared with one another, and numerous analogies between the hydroid gonosome and the inflorescence of plants were demonstrated.

3. On Green's and other Allied Theorems. By Prof. Tait.

(*Abstract.*)

In this paper an attempt is made to supply, at least in part, what the author has long felt as a want in the beautiful system of quaternions, so far as it has yet been developed. To apply it to general inquiries connected with electricity, fluid motion, &c., we require to have means of comparing quaternion-integrals taken over a closed surface with others extended through the enclosed space—and of comparing integrals taken over a non-closed surface with others extended round its boundary. The author recently found that he had already, in the *Quarterly Math. Journal*, and in the *Proc. R. S. E.*, furnished the means of attacking the problem.

By very simple considerations it is established that

$$\iiint \mathrm{S}\nabla\sigma \, d\varsigma = \iint \mathrm{S}.\, \sigma \mathrm{U}\nu \, ds,$$

where ∇ is Hamilton's operator,

$$i\frac{d}{dx} + j\frac{d}{dy} + k\frac{d}{dz},$$

σ is any vector-function of the position of a point, $d\varsigma$ an element of volume, ds an element of surface, ν the normal at ds; and the integrals are extended respectively through the content, and over the bounding surface, of a closed space Σ.

From this equation Green's Theorem is deduced in the form

$$\iiint S.\nabla P\nabla P_1 d\varsigma = -\iiint P_1\nabla^2 P d\varsigma + \iint P_1 S.\nabla P U\nu\, ds,$$

$$= -\iiint P\nabla^2 P_1 d\varsigma + \iint P S.\nabla P_1 U\nu\, ds.$$

Some sections are devoted to the representation of

$$\iiint q d\varsigma$$

(where q is any quaternion whatever) by a surface-integral, and the arbitrary part of the solution in the equation

$$\iiint \tau d\varsigma = \iint ds\, S(U\nu\nabla^{-1})\tau,$$

where τ is any vector, is explained.

It is next shown that, if ρ be the vector of a point, σ and ν as before, we have the equation

$$\int S\sigma d\rho = \iint S.\nabla\sigma U\nu.ds,$$

expressing an integral taken over a limited and non-closed surface by another taken round its curvilinear boundary. That some such representation is possible is obvious from the fundamental theorem above, which shows that for a closed surface

$$\iint S.\nabla\sigma U\nu.ds = \iiint S\nabla^2\sigma d\varsigma = 0,$$

and therefore the surface-integral must have the same value (with a mere change of sign depending on the difference between *outside* and *inside*) for the two parts into which the surface is divided by any closed curve drawn upon it.

Other theorems of a similar character are given, such as

$$\int V\sigma d\rho = -\iint ds\, V.(V.U\nu\nabla)\sigma,$$

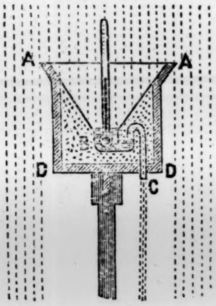
and

$$\int P d\rho = \iint ds\, V.U\nu\nabla P,$$

which, in fact, contains the two preceding.

4. Proposed Method of ascertaining the Temperature of Falling Rain. By Thomas Stevenson, F.R.S.E., Civil Engineer.

A friend informed me some time ago that the late Principal J. D. Forbes had often noticed that a long continuance of rain resulted in a track of cold weather. Principal Forbes attributed this fact to the rain having a lower temperature than the atmosphere through which it fell. It does not appear, however, that he made any observations to determine the truth of his hypothesis, and as the subject is of considerable importance in other meteorological questions, it occurred to me that a simple instrument could be made for ascertaining the temperature of falling rain. This instrument, a rough model of the funnel of which was lately shown at a meeting of the Scottish Meteorological Society, is represented in the accompanying diagram, in which A B C is a conical funnel of thin glass, terminating in a small tube deep enough to contain the bulb of a thermometer, and recurved so as to form an off-let or waster. A D D A represents a box of wood into which the glass funnel is inserted, the space between the glass and the wood being carefully filled with sawdust or any other bad conductor of heat. The rim of the funnel should be bent over the upper edges of the box, so as to prevent the possibility of rain lodging itself among the sawdust.* The rain-drops intercepted by the funnel will pass off through the bottom of the box by the tube C.



By this or some such simple arrangement the temperature of any heavy fall of rain may be ascertained with tolerable accuracy. It is, of course, necessary that a dry bulb thermometer, properly protected by a louvre boarded box, should be observed simultaneously with the *rain thermometer*.

The difference of temperature between the air and rain could also be ascertained by means of an instrument on the principle of Leslie's differential thermometer, one bulb of which would be placed at the bottom of the glass funnel, while the other would be protected from the rain. In this way the differences of temperature would be constantly shown by means of a single instrument.

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* It may be found better to carry the tube, at the second curve, horizontally through the side of the box instead of downwards.

The following Gentlemen were elected Fellows of the Society:—

JAMES WATSON, Esq.
The Hon. Lord MACKENZIE.

Monday, 6th June 1870.

DR CHRISTISON, President, in the Chair.

The Secretary read the following letter from Professor W. J. Macquorn Rankine:—

DIAGRAMS OF FORCES IN FRAMEWORK.

To the Secretary of the Royal Society, Edinburgh.

SIR,—As Mr Clerk Maxwell, in a paper lately published in the Transactions of this Society, has done me the honour to refer to me as having been the first to show how to combine in one diagram a system of lines representing the directions and magnitudes of all the forces acting in a given frame, I wish to put on record, in the Proceedings of the Society, the time and manner of my first publication of the method in question. It was in the year 1856, in a lithographed synopsis of lectures which I delivered in the University of Glasgow, entitled "Mechanical Laws, Formulæ, and Tables." Copies of that synopsis were distributed to the students of my class, and to a few men of science.

I beg leave herewith to send for presentation to the Society a copy of the first part of that synopsis, and regret that at present I am unable to make up a complete copy. The construction of diagrams of forces for unbraced frames is shown at p. 7, and for braced frames at p. 8.

The next publication of the method took place in 1857, in the article "Mechanics Applied," of the "Encyclopædia Britannica," eighth edition; and the next again in 1858, in a work of mine entitled "A Manual of Applied Mechanics."

Mr Clerk Maxwell made a material improvement in the mode of applying the method to *braced* frames, which he published in the "Philosophical Magazine" for 1866, and described to the Dundee meeting of the British Association.—I am, Sir, your most obedient servant, W. J. Macquorn Rankine.

Glasgow, 2*d June* 1870.

The following Communications were read:—

1. On Spectra formed by Doubly Refracting Crystals in Polarised Light. By Francis Deas, Esq., LL.B., F.R.S.E.

The instrument used in the experiments forming the subject of this paper was a spectrum microscope, to which a polarising apparatus is attached, consisting of two Nicol's prisms, each of which is capable of being turned through any required number of degrees.

The first part of the paper relates to the spectra obtained when one or more thin films of mica or selenite are interposed between the polariser and the dispersion prisms, the light being subsequently analysed.

The method employed was, having first determined the axes of the films, to place them on the stage of the instrument which is rotatory, and to adjust them at various angles to the plane of polarisation.

The general appearance presented, may be described as being a more or less continuous spectrum, interrupted by one or more well-defined black bands, not unlike the ordinary absorption bands produced by many chemical substances.

The bands have in many cases a curious movement along the length of the spectrum as the analyser is turned. Sometimes a band may be observed to split into two halves, which move in opposite directions, and unite with other bands which advance to meet them.

In all cases a set of complementary bands is obtained when the plane of analysation has been turned through 90° to that of polarisation. The positions and relative movements of the bands depend partly on the thickness of the films, partly on the inclination of their axes to one another, and to the planes of polarisation as detailed in the paper.

Curious varieties of the movements are obtained by circularly polarising the light before or after its passage through the film.

Very beautiful results were further obtained by substituting a double image prism as the analyser. When the spectra thus obtained are superposed, the bands are no longer black, but coloured, each band in the one spectrum being of the colour of that part of the other spectrum on which it is superposed, while the adjacent colours are those arising from the blending of the two spectra.

To obtain these effects in perfection, however, certain adjustments of the apparatus must be attended to, which will be found described in the paper.

The second part of the paper relates to the effects obtained when a section of a doubly refracting crystal, cut perpendicular to its axis, so as to give the well-known systems of coloured rings, is substituted for the mica or selenite in the former experiments.

The crystal must in this case be placed, not upon the stage, but immediately over the eye lens of the instrument, and between it and the analyser. The entire length of the spectrum is now seen intersected by a system of black arcs, accompanied by two or more brushes, which are black or coloured according to the position of the analyser.

Interesting effects are produced upon the rings by interposing films of mica of different thicknesses, so as to polarise the light either circularly or elliptically; the mode in which the black and coloured rings alternate and change places during the revolution of the analyser depending on the thickness of the film used.

The effect of the rings, when viewed through a double image prism, is strikingly beautiful. Exquisite patterns resembling tessalated pavement, chain armour, &c., may thus, with a little ingenuity in the mode of arrangement, be produced by the interlacing systems of rings.

2. On the Heat Disengaged in the Combination of Acids and Bases. Second Memoir. By Thomas Andrews, M.D., F.R.S., Hon. F.R.S.E.

(*Abstract.*)

In the beginning of this paper the author recapitulates the five fundamental laws of the heat of combination, which he had deduced from his previous researches, and which form the subject of several memoirs published in the Transactions of the Royal Irish Academy and of the Royal Society of London, from 1841 to 1848. They are as follows:—

Law 1.—The heat disengaged in the union of acids and bases is determined by the base, and not by the acid; the same base producing, when combined with an equivalent of different acids, nearly the same quantity of heat; but different bases, different quantities.

Law 2.—When a neutral is converted into an acid salt by combining with one or more atoms of acid, no change of temperature occurs.

Law 3.—When a neutral is converted into a basic salt by combining with an additional proportion of base, the combination is accompanied with the evolution of heat.

Law 4.—When one base displaces another from any of its neutral combinations, the heat evolved or abstracted is always the same, whatever the acid element may be, provided the bases are the same.

Law 5.—When an equivalent of one and the same metal replaces another in a solution of any of its salts of the same order, the heat disengaged is always the same, but a change in either of the metals produces a different disengagement of heat.

The concluding part of the elaborate memoir of MM. Favre and Silbermann, on the heat disengaged in chemical actions, which appeared a few years later, is chiefly devoted to a repetition of the experiments already published by the author. They state that they consider the fourth law, which asserts the equality of thermal effect in basic substitutions, to be fully established; but they dissent from what they consider to be the enunciation of the first law, and infer from their own experiments that the organic acids—

oxalic, acetic, &c.—disengage sensibly less heat in combining with the bases than the nitric, hydrochloric, and other mineral acids. In his first memoir (published in 1841) the author of this communication had, on the contrary, found that the oxalic acid disengages quite as much heat as the nitric and hydrochloric acids, when it combines with the bases, and this property of oxalic acid he always regarded as the key to his whole investigations on this subject. He therefore considered it important to institute a new set of experiments in order to settle the question. These experiments, which were performed with great care, and with accurate instruments, are fully described in the present communication. The results confirm the general accuracy of his original experiments of 1841. They show that oxalic acid, far from disengaging sensibly less heat than the hydrochloric and nitric acids in combining with the bases, actually disengages a little more heat than either of those acids, when it combines with potash, soda, or ammonia. The following extract from a table given in the present communication will illustrate this point:—

Acid.	Potash.	Soda.	Ammonia.
Oxalic, . .	3°·058	3°·040	2°·648
Hydrochloric, .	3°·021	2°·982	2°·623
Nitric, . .	2°·993	2°·929	2°·566

The original experiments of the author, according to which oxalic acid stands, as regards thermal action, in the same rank as the phosphoric, nitric, arsenic, hydrochloric, hydriodic, boracic, and other mineral acids (with the exception of the sulphuric acid), are thus completely confirmed. The new experiments also agree with the former ones in showing that sulphuric acid disengages about $\frac{1}{8}$th more heat, and a group of acids comprising the tartaric, citric, and succinic acids, about $\frac{1}{30}$th less heat than the mean of the other acids. The results are fully discussed in the present memoir, and the influence of extraneous circumstances considered, which in this, as in other similar physical inquiries, disturb in all cases to a certain extent, and in some cases considerably, the experimental indications, and render them only first approximations to the general laws they are designed to illustrate.

3. Note on Professor Bain's Theory of Euclid I. 4. By Wm. Robertson Smith, M.A., Assistant to the Professor of Natural Philosophy. Communicated by Professor Tait.

In a paper communicated to this Society last session, I pointed out that the proof of Euc. I. 5, given by Mr Mill, is unsound; endeavouring, at the same time, to show that this is no mere accident, but that it is impossible to give a mathematically correct analysis of the processes of Synthetic Geometry on any theory that holds figures to be merely illustrative, and does not admit that intuition in the Kantian sense—*i.e.*, actual looking at a single engraved or imaginary figure—may be a necessary and sufficient step in a demonstration perfectly general. I now venture to draw the attention of the Society to the confirmation which I conceive that this argument derives from the way in which Euc. I. 4 is treated by Professor Bain in his recent "Logic"—a book which, on the whole, is based on Mr Mill's principles, and which is mainly original in an attempt, which I cannot regard as felicitous, to bring these principles into closer contact with the special sciences, especially with Physics and Mathematics.

It will be remembered that Mr Mill, undertaking to demonstrate Euc. I. 5 from first principles, has to supply, in the course of his proof, a demonstration of Euc. I. 4, and it is in the attempt to give to this process the form of syllogistic inference from Euclid's axioms that he errs. Professor Bain does not attempt to defend the blunder of his predecessor. He admits that Euclid's proof cannot be reduced to a chain of syllogisms. But, instead of surrendering Mr Mill's theory of mathematical reasoning, he concludes that Euclid has not demonstrated his proposition—that the superposition which he enjoins is only an experiment, and that "if his readers had not made actual experiments of the kind indicated, they could not be convinced by the reasoning in the demonstration." *

Now I believe, and in my former paper expressly pointed out, that the position that Euc. I. 4 is really an inductive truth, and that the usual demonstration is not in itself convincing, is the only ground that remains to Mr Mill and his adherents. So far, then, I am confirmed by Professor Bain: it remains only to show that this new position is mathematically as untenable as that from which Mr Mill has been dislodged. If Professor Bain grants that the proof of Euc. I. 4 is not by syllogism from axioms—if, again, mathematically it is plain that there is none the less a real proof, not merely an induction—we shall have gone far to establish the validity of proof by intuition.

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* Logic, vol. ii. p. 217.

Professor Bain tells us that Euclid, while professedly going through a process of pure deduction, requires us to conceive an experimental proof. There is surely an ambiguity here. Does Mr Bain mean that Euclid merely calls to our mind former concrete experiments with triangles of card-board or paper, for these alone are actual and concrete to our author? Does Euclid's "experiment" agree with the descriptions of experiments in books of Physics, save only in this, that we have all made Euclid's experiment before? Clearly not. In picturing to myself an experimental proof in the usual sense, I imagine mentally, or with the help of a diagram, certain arrangements, and then I am told to imagine a certain result following—or rather, I am told to believe this result, for to picture it is quite superflous and often impossible. Euclid, on the other hand, tells me to superpose ideally the point A on C, the line AB on CD, and so forth, and then I do not require to be told that the coincidence of the whole triangles follows. I have no choice to imagine coincidence or non-coincidence. I see that it follows, and that quite apart from previous experiment.

Professor Bain allows the possibility of ideal experiments on mathematical forms.* I presume, therefore, that he will not deny that the intelligent reader of our proposition does, as he reads, make a valid experiment in favour of the proposition. But if this be so, where is the deception in Euclid's proof, and what is the necessity of supplementing that proof by further "ideal" or "actual experiments"? The course of Euclid's argument shows that the two triangles are not only equal, but equal in virtue of the way in which they have been constructed, viz., the equality of the two sides and the included angle. The fact that the proof is not syllogistic does not make it any the less a case of that parity of reasoning which Professor Bain, in another connection, admits to be not induction but demonstration.*

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* Logic, vol. i. p. 225.

Our author draws a broad line between the fourth proposition, with its "appeal to experiment or trial in the concrete," and the mass of geometrical proofs in which the figure is referred to for verification only, "the effect of every construction and every step of reasoning being judged of by actual inspection." But if the inspection follows the construction, what is the construction itself? A construction is not proved by syllogism from axioms. It is necessarily drawn, and in the drawing (mental or other) looked at. Every construction involves a figure and an intuition, which, while it looks at the individual figure, sees in it the general truth.† Mr Bain grants that of such consequences as that the diagonal of a parallelogram divides it into two triangles, Euclid offers no other proof than an appeal to the eye.‡ In fact, no other proof can be offered. Yet surely it will not be asserted that this too is an induction. In one word, if no proposition is fairly demonstrated where it is essential to look at the figure, there is no sound demonstration in synthetic geometry.

Finally, Professor Bain himself seems not quite satisfied as to the inductive nature of Euc. I. 4. "The proof," he says, "rests solely on definitions," and hence "the proposition cannot be real—the subject and predicate must be identical." Surely an identical proposition is not an induction! And surely, too, the proof rests not on definitions merely, but on definitions and the use of the figure! But I do not think that Professor Bain means to speak here in strict logical terms, for he straightway adds in explanation, "The proposition must, in fact, be a mere equivalent of the notions of line, angle, surface, equality—a fact apparent in the operation of understanding these notions. It is implicated in the experience requisite for mastering the indefinable elements of geometry, and should be rested purely on the basis of experience." We should have known better what this sentence means, if the author had adopted here the distinction between synthetic and analytic judgments. He cannot mean that a truth that is an induction, and rests on experience, is an analytic judgment, that it can be reached by a purely formal dividing and compounding of the definitions of terms. Such a proposition could be shown to be true without any figure or any experiment. Yet the proposition is, we are told, involved in the notions; we cannot know what lines, angles, &c., are without knowing this too. If this means anything, it means that Euc. I. 4 is a synthetic judgment *a priori*; and that, after all, Kant and the mathematicians are right, and Mr Mill and the empirical logicians wrong.

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* Logic, vol. ii. p. 5.

† *Cf.* Kant, Krit. d. r. Vern. p. 478. Ed. Hartenstein, 1867.

‡ Logic, vol. ii. p. 218.

4. A Simple Mode of Approximating to the Wave-Length of Light. By W. Leitch, Assistant to the Professor of Natural Philosophy in the University of Glasgow. Communicated by Professor Tait.

The fundamental phenomenon or fact of the science of optics is vision, that is, the perception we have of distant objects through the eye, or by the sense of sight. That vision is an effect transmitted to the mind by the object seen, is a necessary truth, involved in the definition of the term, and independent of all theoretical views beyond the consciousness of that perception.

Common observation informs us that vision cannot take place without that which we call light, and that light itself cannot exist without the presence of a self-luminous body. Every one has a distinct conception of the meaning of the terms light and luminous; their definition according to that conception would be a verbal exercise of no utility at present.

Next may be placed the fact, first revealed by astronomical observations, and afterwards verified by other experiments, that light is not transmitted instantaneously,—in other words, that some portion of time elapses between the occurrence of a visible phenomenon and our perception of it by the eye, such as, for simplicity, the passage of an electric spark, or the occultation of a star by the dark body of the moon or of a planet; and that the portion of time in question is in direct proportion to the distance of the object seen from the eye, the intervening medium being the same.

The progressive motion of light from the object seen to the eye being established, and the supposition that it is a substance emanating from the object with the velocity found, being seen to be inconsistent with the phenomena of interference, we can scarcely be said to make use of a hypothesis when we conclude that it is an action transmitted through a medium bodily at rest, it may be, but whose component molecules act upon one another in such a way as to propagate the effect in question. By the term light we mean this action considered as a physical fact, separate from our perception of it by the eye, and independent of its arrival or non-arrival at our organs of vision.

The propagation of light from a luminous point with the same velocity in all directions (in a homogeneous medium), implies that the action originating at any instant in the source is diffused over a spherical surface whose radius, measured from the luminous point as centre, constantly increases at the rate of the velocity of light; and the constancy with which this propagation is kept up, implies that there are an infinite number of such spherical surfaces, over each of which is diffused an action which originated in the source at a preceding instant. Next the question presents itself whether all these actions originating in the source at successive instants, and occupying successive spherical surfaces, are similar and equivalent. The phenomena of interference answer, that if we imagine a series of these spherical surfaces separated from each other by a very small constant distance λ, the action propagated upon each of these surfaces is the same, and that midway between each pair of the series is a surface propagating an action capable of destroying that of its neighbour of the first series, from which it is separated by the constant distance $\frac{\lambda}{2}$. Now, that is equivalent to saying that each thin spherical shell of the medium through which the action is transmitted, vibrates between opposite phases, and as it is impossible for us to conceive or believe that any finite change can take place in the material world that does not involve an infinite number of intermediate infinitesimal changes, we are authorised to say that light consists in periodic vibrations, propagated with very great velocity, and decomposable in an infinite number of ways into half vibrations exactly contrary to one another.

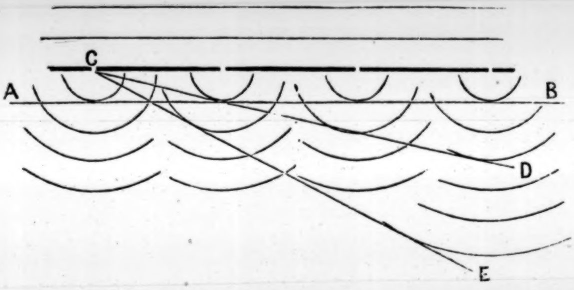
Thus far we have arrived without having recourse to any hypothesis, having assumed nothing regarding the nature of these vibrations, the word vibration being understood in its most general sense as meaning oscillation between opposite phases or conditions, a fact revealed to us by the phenomena of interference. Even at this point, however, the hypothesis which forms the basis of the undulatory theory cannot fail to present itself to our minds, the hypothetical part being not so much the existence of a medium, or the propagation of vibrations, but the nature attributed to these vibrations, viz., that they consist in mere mechanical action, involving nothing but variations of pressure and displacement among the particles of which the medium is composed, and propagated according to the same laws as in ponderable media with which we are more familiar. The suspension of interfering vibrations is interpreted in the simplest manner as the result of the simultaneous application of equal and opposite forces, or according to a fiction easily understood, the superposition of equal and opposite motions, and their reappearance after separation as the natural consequence of the indestructibility of force. Moreover, our experience does not enable us to conceive any other kind of vibrations decomposable in the same manner, though the phenomenon of electrolysis seems to indicate the propagation of a periodic oscillation between opposite phases of decomposition and recomposition, involving something more than variations of pressure and displacement among the particles of water. Even the small degree of uncertainty that may remain at this stage of the inquiry, is diminished by the phenomenon of diffraction, and by the physiological analogy between the eye and the ear, both of them situated like feelers of the brain; we know the variety of perceptions that are communicated to the mind by the effect of mechanical vibrations upon one of these organs.

Adopting the hypothesis, we call these vibrations waves, from their analogy to the vibrations so designated in the case of water, and the distance λ above mentioned we call the length of a wave of light. In order to effect its measurement, we produce the phenomenon of interference; that is done most directly by deflecting two pencils of light proceeding from the same source in such a way that they may be superposed after traversing paths differing by $\frac{\lambda}{2}$, or $\frac{3\lambda}{2}$, $\frac{5\lambda}{2}$, &c.; but the most instructive method is to produce the phenomenon of diffraction, which is usually accompanied by that of interference.

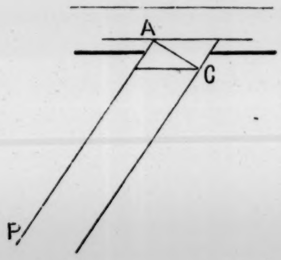
Diffraction is the name given to the lateral deviation of light in passing the edge of an obstacle, *i.e.*, of an opaque body. Having adopted the undulatory theory, we are ready to admit that such a deviation ought to take place, both from our experience of similar effects in air and water, and from our general ideas of the structure and equilibrium of fluids, from which we conclude that no single particle of a fluid can be disturbed without disturbing those surrounding it on all sides, that is, propagating a disturbance in all directions. When light, proceeding from a luminous source of very small apparent diameter, passes the edge of a dark body and is received upon a screen, instead of a sudden transition from light to darkness at the line where the geometrical shadow commences, we observe a gradually diminishing illumination for some distance inside of that line, and outside of it we observe maxima and minima of illumination arranged in bands parallel to it, if it is a straight line. In order to effect the measurement of the length λ, and understand the principle of the process, it is not necessary to follow the mathematical investigation of the position and intensity of these maxima and minima. That investigation is based upon the axiom that each point of a wave of light is a centre of force, the molecule there situated tending to propagate the energy with which it is animated in all directions around it, so that, if it were at any instant the only molecule agitated, it would immediately become the actual centre of a spherical wave. In the case of the uninterrupted propagation of a spherical wave, it is the envelope of all these elementary undulations to which is transmitted the vibratory movement of each molecule, and which, by reason of symmetry, is a spherical surface concentric with that which it succeeds. Diffraction takes place when part of the wave is intercepted by an obstacle, and the symmetry is destroyed which kept the surface of the wave concentric with its first position. The propagation of a spherical wave does not require that contiguous molecules be allowed free play. If we look at a luminous source through a fine grating, we see it in the same position as if the grating were removed, which proves that a concentric spherical wave is formed by the union of the fragmentary parts of the incident wave which the grating has allowed to pass, or at least the fragmentary parts distributed over the spherical surface produce the same effect upon our sense of vision as if the surface were occupied by an unbroken wave. If the grating be sufficiently fine, and the luminous source not too near, we see not only the source in its proper position, but also images of it on both sides in the direction at right angles to the wires or dark lines of the grating. If the light of the source be homogeneous, that is, the same as we find at any point of a pure spectrum, these lateral images are counterparts of the true image, of various intensities. If the source emit white light, it is exhibited in each of these images separated into its component colours, the image being spread out so as to form a spectrum, with the violet extremity nearest to the central image.

In order to understand the origin of these lateral images, first suppose the transparent intervals to be of infinitely small width, and separated by dark spaces of finite and equal breadth. Suppose light coming from a distant source to be incident upon them in a direction perpendicular to their plane. The space occupied by the system of lines and spaces being very small, the surface of an incident wave may be considered as coinciding with their plane, so that a similar phase of vibration passes at all points of the transparent lines at the same instant. Each of these lines thus becomes the axis of a system of cylindrical waves behind the grating, and at any instant the same phase of vibration is found in each system at the same distance from the axis.

Suppose the dark lines in the figure to represent sections of these cylindrical surfaces in the same phase of vibration. Upon the surfaces which envelope a succession of these surfaces of similar phase, in a direction parallel to AB, are formed a system of waves by which we see the true image in its real position; similarly, by a system of waves which envelope surfaces of similar phase, in a direction parallel to CD, we see the first lateral image to the right; by a system of waves parallel to EC, we see the second image, and so on. If we denote by a the distance between the transparent lines, and by D, the angular deviation of the first lateral image, we find, from the position of the surface CD, a sin D as the distance between successive surfaces of similar phase parallel to CD, that is to say, as the length of the wave of the light propagated in the direction normal to CD. Similarly, by drawing perpendiculars upon the successive envelope surfaces through C from the first opening to the right, we get for the same wave length $\frac{1}{2}$ a sin D_2 from the second image, $\frac{1}{3}$ a sin D_3 from the third, and so on. In the case of white light, the separation into its component colours exhibited in each lateral image enables us, by observing the deviation of each colour of the spectrum, to measure the wave length of light of that colour.

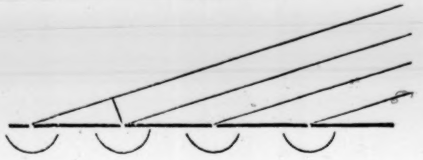


The lateral images are thus easily accounted for in the imaginary case, in which the transparent intervals are of infinitely small breadth. Gratings have been constructed by ruling sensibly dark lines upon glass so closely that the breadth of the transparent interval is only a small fraction of the length of wave. The explanation of the images seen through these is the same as that just given for the imaginary case. Suppose, however, the width of the spaces to be so much greater than the length of wave, that the small inclined surface AC which covers the opening, as seen in the direction AP normal to AC, stretches obliquely across the exact length of a wave of the incident light, the surface AC, which would be the locus of the same, or at least concordant phases of vibration if light were propagated in the direction AP,

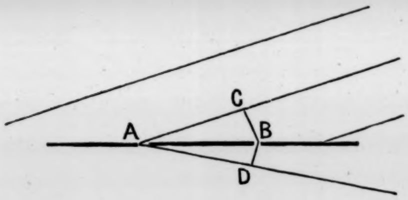


will contain nothing but a series of equal and opposite phases, which will be discordant and mutually destructive, as far as concerns the propagation of light in the direction AP, and no image will be seen in that direction, whatever may be the distance between the transparent spaces. The same will be the case if the breadth of the spaces be such that the surface AC stretches across exactly 2, 3, or any whole number of wave lengths. But if the surface AC stretches across $n + \frac{p}{q}$ wave lengths, $\frac{p}{q}$ being a proper fraction, the vibratory movement transmitted along AP by the fractional part of the wave length will not be destroyed by the concurrence of its complete opposite, and light will be propagated along AP. The other transparent spaces will send concordant phases to the envelope wave, if AP be at the proper angle. In this case, however, the breadth ϵ of a transparent space must be added to a in the formula a sin D, &c., $a + \epsilon$ being the distance between the successive effective remnants of the vibratory movements which pass to the envelope surfaces. The breadth $a + \epsilon$ occupied by a dark and a transparent space is called an element of the grating. If the fractional part $\frac{p}{q}$ of the wave length, which is effective in forming any one of these envelope waves, be either a very small or a very large fraction, its effect will be feeble, and the corresponding image of small intensity; but if it be exactly one-half of the wave length, its effect will be the greatest possible, and the envelope wave will receive from each opening the greatest possible amount of concordant action. In this manner is explained the difference of intensity of these lateral images, the one nearest to the central image not being always the brightest. Proximity to the central image is, however, also a cause of greater brightness, it being evident that the less the surface AC in the last figure is inclined to the incident waves, the greater is the absolute length of that part of it which stretches over any given fraction of the wave length, and the greater the amount of action of which it is the locus.

In the above the incident waves have been supposed to be exactly parallel to the plane of the grating, so that the same phase of vibration passes at the same instant through all the openings.



The figure annexed shows that if the incident waves be inclined to the grating at such an angle that the perpendicular from any opening upon the wave surface passing through the next opening is equal to the wave length, the same phase will in this case also pass all the openings at the same instant, though derived from different incident waves, and the first lateral image will be seen in a direction normal to the grating. The same formula will give the wave length in this case, D being always the angular deviation from the true image or from the direction of the incident light. This is the condition approximately realised in the arrangements for measuring the wave length about to be described, but as no provision is made for an exact adjustment of the grating to the inclination just indicated, and as a very minute error in such an adjustment would cause the conditions of the experiment to be altogether different from those indicated by the figure above, it is necessary to account for the appearance of lateral images in the case of light incident at any angle, and find a formula for the wave length applicable to that case.



If, as in the figure below, the incident waves be so inclined to the grating that the perpendicular BC, together with the perpendicular BD, make up the wave length, the same phase of vibration will be situated at A and D; for the same reason, behind every two consecutive openings, like phases will be situated upon surfaces inclined at the same angle as AD, that is to say, AD produced will envelope like phases, and the first lateral image will be seen in the direction normal to AD. If we denote by I the angle of incidence CAB, and as before by D, the angle of deviation CAD, we get $\lambda = (\alpha + \epsilon)\{\sin I + \sin(D - I)\}$. So long as I and D are small, the latter factor is approximately $= I + D - I = D = \sin D$, the same as before, so that in that case the error introduced by using the formula first obtained with only an approximate adjustment of the grating is inconsiderable. The same is the case if either I alone or D − I be very small.

By differentiating the formula we get

$$\cos \mathrm{I} + \cos (\mathrm{D} - \mathrm{I}) \left(\frac{d\mathrm{D}}{d\mathrm{I}} - 1 \right) = 0 \quad . \qquad (2).$$

$$\therefore \frac{d\mathrm{D}}{d\mathrm{I}} = \frac{\cos (\mathrm{D} - \mathrm{I}) - \cos \mathrm{I}}{\cos (\mathrm{D} - \mathrm{I})} = 1 - \frac{\cos \mathrm{I}}{\cos (\mathrm{D} - \mathrm{I})},$$

$$\text{(and if } \mathrm{D} = 2\mathrm{I}) \qquad = 1 - \frac{\cos \mathrm{I}}{\cos \mathrm{I}} = 0,$$

that is, D is constant for small variations of the position of the grating, or angle of incidence, while the variation of the latter by condition (2) does not affect the value of λ calculated from the formula. There is, therefore, an advantage in observing with the grating adjusted to bisect the angle between the directions of incidence and diffraction, that being the position in which a small error in the adjustment has the least effect upon the result given by the formula, which becomes in this case,

$$\lambda = 2(a + \epsilon) \sin \frac{\mathrm{D}}{2} .$$

In the arrangements now to be described, in which we use two sources of light, one on each side of the normal to the grating, we make the angle (D − I) approximately vanish, and use the mean of the two angles of incidence in the formula

$$\lambda = (a + \epsilon) \sin \mathrm{I}.$$

By neglecting the part $(a + \epsilon) \sin (\mathrm{D} - \mathrm{I})$, which is positive for the one light, and negative and of the same magnitude for the other, as is plain from the method of observing, we introduce no error into the result.

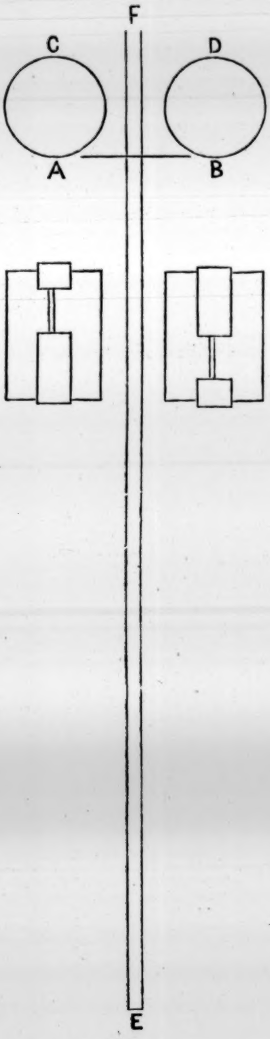
AC, BD, are sections of two rectangular pieces of tin bent into a cylindrical form round the glass funnels of two paraffin lamps. Their edges come short of meeting so as to leave a slit at A and B of about 1 millimetre in breadth. These slits are partially covered with tin as shown immediately below, where they are drawn as they appear to the eye of the observer. A thread is stretched round the two cylinders, partly shown between A and B. EF is a straight stick passing horizontally immediately under the thread, and graduated in centimetres on its upper edge. A grating constructed by drawing transparent lines at the rate of 2000 to the inch upon glass covered with a dark ground, is held by the hand against the end E of the stick, cut square with its edges. The stick is then pushed in or out in the direction of its length till the red colour of the first spectrum to the right of A is seen to be directly above the same colour of the first spectrum to the left of B. A pencil mark is then made upon the stick directly below the thread. The stick is then drawn further out until the yellow colours of the two spectra are seen in the same vertical line, and another mark is made; and so with the remaining colours. The distance from centre to centre of the two slits, in a horizontal line, being 10 centimetres, the distances marked off between E and the thread were read 99·5 centimetres for the red, 107 for the yellow, 116 for the green, and 135 for the blue. These numbers were taken for the distance to the light in each case, being only about $\frac{1}{50}$ per cent. less by calculation. The corresponding wave lengths by the formula $\lambda = (\alpha + \epsilon) \sin I$, are

Red . $\frac{5}{99\cdot5} \times \frac{1}{2000} = \frac{1}{39800}$ of an inch = ·000638 millimetre.

Yellow $\frac{5}{107} \times \frac{1}{2000} = \frac{1}{42800}$ „ = ·000593 „

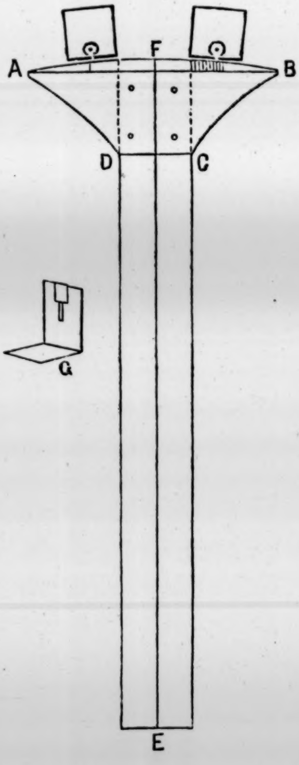
Green $\frac{5}{116} \times \frac{1}{2000} = \frac{1}{46400}$ „ = ·000547 „

Blue . $\frac{5}{135} \times \frac{1}{2000} = \frac{1}{54000}$ „ = ·000470 „



Different measurements may be got by the same observer at different times from his uncertainty as to the points in the spectrum at which he should consider each colour to begin and end. This uncertainty is usually considered to be obviated by using solar light, and measuring the deviations of the dark lines in the spectrum; but as these lines are the parts of the spectrum from which no light comes, the process can scarcely be called the determination of the wave length of light.

Since the above measurements were made, an improvement was made in the apparatus by which the gratings were constructed, and finer gratings were made, which gave more brilliant spectra, by reason of the greater number of apertures from which similar phases of vibration came to the eye. With one of these, consisting of transparent spaces drawn at the rate of 3000 to the inch, a new set of measurements was taken in the following manner:—



EF represents a rectangular piece of wood upon which is pinned a piece of paste-board ABCD, whose edge AFB is an arc of radius 20 inches and centre at E. The chord AB is divided into tenths of an inch by perpendiculars to it meeting the arc. Touching the arc are placed, but not fixed, two pieces of tin bent as represented at G, each having a narrow slit so situated that the bottom of the one slit is on a level with the top of the other, and carrying a small piece of candle immediately behind the slit. The grating is held at E, and the pieces of tin are moved along the arc until the colour observed in each spectrum is in the same vertical line at F. The distance between the two slits is then read upon the graduated chord, and the half of that distance divided by 20 inches is the sine of the deviation. In this case the second spectrum from each light was observed, and the observed distances for the red, yellow, green, and blue, were 5·9, 5·5, 5·025, and 4·25 inches respectively. The wave lengths calculated from these data are in millimetres—

$$\frac{1}{2}\cdot\frac{5{\cdot}9}{2}\cdot\frac{1}{20}\cdot\frac{25{\cdot}4}{3000} = {\cdot}000624 \text{ millimetre for the red,}$$

$$\frac{1}{2}\cdot\frac{5{\cdot}5}{2}\cdot\frac{1}{20}\cdot\frac{25{\cdot}4}{3000} = {\cdot}000582 \quad\text{,,}\qquad\text{,,}\quad\text{yellow,}$$

$$\frac{1}{2}\cdot\frac{5{\cdot}025}{2}\cdot\frac{1}{20}\cdot\frac{25{\cdot}4}{3000} = {\cdot}000531 \quad\text{,,}\qquad\text{,,}\quad\text{green,}$$

$$\frac{1}{2}\cdot\frac{4{\cdot}25}{2}\cdot\frac{1}{20}\cdot\frac{25{\cdot}4}{3000} = {\cdot}000449 \quad\text{,,}\qquad\text{,,}\quad\text{blue.}$$

The apparatus contrived and constructed by the author to produce these fine gratings has not been described, because its construction involves considerable trouble and expense, which the experimenter may avoid by applying to an instrument-maker who has a dividing machine. The difficulty of getting a sufficiently fine dark ground upon the glass will also be avoided if the dividing machine be fitted with a diamond point, which will scratch comparatively opaque lines on the transparent surface of the glass. The finest gratings constructed are produced in that way.

5. Note on Linear Partial Differential Equations. By Professor Tait.

The equation

$$P\frac{du}{dx} + Q\frac{du}{dy} + R\frac{du}{dz} = 0$$

may be put in the very simple form

$$S(\sigma\nabla)u = 0,$$

if we write

$$\sigma = iP + jQ + kR,$$

and

$$\nabla = i\frac{d}{dx} + j\frac{d}{dy} + k\frac{d}{dz}\,.$$

This gives, at once,

$$\nabla u = mV\theta\sigma\,,$$

where m is a scalar and θ a vector (in whose tensor m might have been included, but is kept separate for a special purpose.) Hence

$$\begin{aligned} du &= -\,\mathrm{S}(d\rho\nabla)u \\ &= -\,m\mathrm{S}.\theta\sigma\,d\rho \\ &= -\,\mathrm{S}.\,\theta d\tau\,, \end{aligned}$$

if we put

$$d\tau = m\mathrm{V}.\sigma\,d\rho$$

so that m is an integrating factor of $\mathrm{V}.\sigma\,d\rho$. If a value of m can be found, it is obvious, from the form of the above equation, that θ must be a function of τ alone; and the integral is therefore

$$u = \mathrm{F}(\tau) = \text{const.}$$

where F is an arbitrary scalar function.

Thus the differential equation of *Cylinders* is

$$\mathrm{S}(\alpha\nabla)u = 0\,,$$

where α is a constant vector. Here $m = 1$, and

$$u = \mathrm{F}(\mathrm{V}\alpha\rho).$$

That of *Cones* referred to the vertex is

$$\mathrm{S}(\rho\nabla)u = 0.$$

Here the expression to be made integrable is

$$\mathrm{V}.\rho d\rho.$$

But Hamilton long ago showed that

$$\frac{d\mathrm{U}\rho}{\mathrm{U}\rho} = \mathrm{V}\frac{d\rho}{\rho} = \frac{\mathrm{V}.\rho d\rho}{(\mathrm{T}\rho)^2}\,,$$

which indicates the value of m, and gives

$$u = \mathrm{F}(\mathrm{U}\rho) = \text{const.}$$

It is obvious that the above is only one of a great number of different processes which may be applied to integrate the differential equation. It is quite easy, for instance, to pass from it to the assumption of a vector integrating factor instead of the scalar m, and to derive the usual criterion of integrability. There is no difficulty in modifying the process to suit the case when the right hand member is a multiple of u. In fact it seems to throw a very clear light upon the whole subject of the integration of partial differential equations. But I have not at present leisure to pursue the subject farther than to notice that if, instead of $\mathrm{S}(\sigma\nabla)$, we employ other operators as $S(\sigma\nabla)S(\tau\nabla)$, $S.\sigma\nabla\tau\nabla$, &c. (where ∇ may or may not operate on u alone), we can pass to linear partial differential equations of the second and higher orders. Similar theorems can be obtained from vector operations, as $V(\sigma\nabla)$.

6. On the Oxidation Products of Picoline. By James Dewar, F.R.S.E., Lecturer on Chemistry, Veterinary College, Edinburgh.

(*Abstract.*)

The author in this paper details the results of a series of experiments, commenced three years ago, on the oxidation of the pyridine series of bases. These bodies are readily attacked by permanganate of potash; and the oxidation products of picoline thus obtained are ammonia, carbonic, nitric, oxalic, acetic, and dicarbopyridenic acids, along with a very small quantity of some solid base, possibly a condensed base.

Dicarbopyridenic acid, $C_5H_3N\begin{matrix}CO_2H\\CO_2H\end{matrix}$, is bibasic, and bears the same relation to the nucleus pyridine that phthalic acid and its isomers bear to benzol. It crystallises from hot aqueous solutions in plates resembling naphthaline; the majority of its salts are soluble and crystallisable. The silver salt of the acid is very characteristic, being insoluble and gelatinous, not decomposed by boiling water, and not visibly affected by light. As this acid was got in only small quantity, the author had not the opportunity of producing its various derivatives.

The author observes that the two well-defined series of nitrile bases found in coal tar, viz., the pyridine and chinoline series, bear the same relation to each other that the benzol series of hydrocarbons does to the naphthaline. Thus, pyridine is supposed to be the nucleus in these bodies that benzol is in the aromatic series. The following are some of the analogies pointed out in the paper:—

Benzol.	Naphthaline.	Anthracine.	Pyridine.	Chinoline.
C_2H_2	C_6H_4	C_6H_4	C_2H_2	C_5H_3N
C_2H_2	C_2H_2	C_2H_2	NCH	C_2H_2
C_2H_2	C_2H_2	C_6H_4	C_2H_2	C_2H_2

Chinoline and pyridine, therefore, ought to be readily obtainable

from each other, and it is the intention of the author to work in this direction. It is observed also that indol, the nucleus of indigo, is benzol-pyrrol, being related thus,

Indol.	Pyrrol.
C_6H_4	C_2H_2
NH	NH
C_2H_2	C_2H_2

It is therefore likely that indol may be met with along with pyrrol among the products of the destructive distillation of nitrogenised organic substances. While this paper is passing through the press, Professor Baeyer of Berlin has pointed out, independently, a similar relation between pyrrol and indol, a note of which has just been published.

7. Notes of some Experiments on the Rate of Flow of Blood and some other Liquids through tubes of narrow diameter. By J. Matthews Duncan, M.D., F.R.S.E, and Arthur Gamgee, M.D., F.R.S.E.

The experiments, of which the results are recorded in the present communication, were undertaken in order to determine the rate at which blood flows through tubes of moderately small diameter, with a view to the study of the mechanical theory of dysmenorrhœa; they were afterwards extended to blood-clot, serum, milk, and urine, &c.

In a memoir inserted in the ninth volume of the "Mémoires des savants étrangers," M. Poiseuille stated the results of an investigation on the flow of water and other fluids through capillary tubes, showing how this is influenced by pressure, by the length and diameter of the tube, and by temperature. A committee of the French Academy, of which M. Regnault was the reporter, corroborated the results of M. Poiseuille's researches.* Subsequently this observer published a still more extended series of observations, including the determination of the rate of flow of serum and defibrinated blood.†

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* Recherches expérimentales sur le mouvement des liquides dans les tubes de très-petits diamètres. Commissaires MM. Arago, Babinet, Piobert, Regnault rapporteur. Académie des Sciences, séance du 26th Décembre 1842.

† Recherches expérimentales sur le mouvement des liquides de nature différente dans les tubes de très petits diamètres par M. le Dr Poiseuille. Annales de Chimie et de Physique. Troisième série t. xxi. 1847.

The method employed by Poiseuille in his researches, and which is described at length in his Memoir, consisted essentially in causing air under a known pressure to force a known quantity of the fluid to be experimented upon through tubes of known diameter and length, and determining the time employed.

The following are the general results to which he arrived concerning the influence of the length and diameter of tubes of smaller diameter than a millimetre on the rate of flow of any liquid at a constant pressure and temperature:—

1*st*. The volumes of liquid flowing in equal times through capillary tubes of equal length, but of different diameters, are amongst themselves as the fourth powers of the diameters.

2*d*. The volumes of liquids which flow in equal times through capillary tubes of the same diameter, but of different lengths, vary inversely as the length of the tubes.

With regard to the influence of pressure, it was found that the rate of flow increased directly as the pressure; and with regard to the temperature, that, *as a general rule*, the rate of flow of solutions increases as the temperature rises.

With regard to the influence of various substances held in solution by a fluid, on the rate of flow, no general law was arrived at, connecting it either with chemical constitution, density, capillarity, or viscosity.*

The following are some of the results, extracted from M. Poiseuille's Memoir—

I. Tube employed (B) is 64 millimetres long; its diameter is $0^{mm}\cdot249$; capacity of receiver, 6 C. C.; pressure, 1 metre; temperature, 14°5 C.

	Time of Flow. s.
1. Distilled water, ...	535·2
2. Ether, ...	160·0
3. Alcohol, ...	1184·5
4. Serum of ox's blood, . . .	1029·0

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* We may merely allude to the fact that M. Graham succeeded in showing a decided connection between the rate of flow of the different hydrates of sulphuric acid and their chemical constitution. His very interesting results are to be found in a paper "On liquid transpiration in relation to chemical composition." (*Philosophical Transactions*, 1861, p. 373).

M. Poiseuille made a single determination of the rate of flow of blood serum; of blood serum plus a small and unknown quantity of corpuscles, and of defibrinated blood, the same animal's blood (an ox's) having been used to furnish the three liquids. The following are the results—

Temperature and pressure stated to have been kept constant during all the experiments; length of tube, 110 millimetres; diameter, $0^{\mathrm{mm}}\cdot256$; capacity of receiver, between 5 and 6 C. C.

	Time of Flow. *m. s.*
Serum, ...	20·33
Serum and a small and unknown quantity of blood corpuscles, . . .	21·17
Defibrinated blood, ...	68·47

Poiseuille points out that the aggregation of blood-corpuscles, which always takes place in defibrinated blood, leads to a choking of the tubes employed, especially when these are of narrow diameter ($0^{\mathrm{mm}}\cdot1$), or to an irregular flow, and that consequently defibrinated blood cannot readily be injected through the capillaries of the lungs of animals which have been bled to death. The recent experiments of Dr J. J. Müller,[*] carried on under the direction, and according to the method, of Professor Ludwig, in the Physiological Institute of Leipzig, are opposed to the statement of Poiseuille, for he succeeded in keeping up for long periods a flow of defibrinated blood through the lungs.

Method employed in the present research.

All experiments were conducted according to a method suggested by, and under the direction of, Professor Tait, in the Physical Laboratory of the University of Edinburgh. The liquids to be experimented upon were allowed to flow through tubes of known diameter and length, into a large air-pump receiver exhausted to a partial and known extent, the fluid being thus subjected to the pressure of the atmosphere, minus that of the air in the receiver.

Before enumerating our experiments, it may be well to point out certain fundamental differences which exist between them and those of M. Poiseuille. 1*st*, Our tubes had a much wider diameter—those used by the French observer varied in diameter from $0^{\text{mm}}\cdot1949$–$0^{\text{mm}}\cdot256$, whilst our tubes were from $0^{\text{mm}}\cdot845$–$1^{\text{mm}}\cdot259$. 2*dly*, By our tubes being much longer than those of Poiseuille; and, 3*dly*, By the liquids being allowed to flow, not into water, but into empty vessels placed in the partially exhausted receiver.

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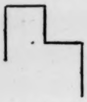
[*] “Ueber die Athmung in der Lunge von Dr J. J. Müller.” Arbeiten aus der Physiolog. Aust. zu Leipzig Mitgetheilt durch C. Ludwig. Leipzig, 1870, p. 37–76.

I.—*Influence of the Shape of the Tubes employed on the Rate of Flow.*

It was considered advisable to determine, in the first place, whether bends in the tubes through which the liquids were made to flow would exert any influence on the rate. Accordingly, a tube 1129 millemetres long was bent twice at right angles; one end was connected by means of a tightly fitting cork with the

Table I.

No. of Experiments.	Fluid used.	Diameter of Tube.	Length of Tube.	Temperature.	Pressure.	Time of Flow of 100Cubic Cents. in Seconds.	
		mm.	mm.	°	mm.		
1–5	Water, . .	0·845	1129·8	13·0 C	708·59	126·4	Tube bent twice at right angles, thus,
5–8	Common Sulphuric Acid,	”	”	13·5	”	2978·0	
8–9	Water, . .	”	”	13·5	588·5	158·0	
10–11	Water, . .	0·845	1129·8	13·5	588·5	159·8	Tube bent four times at right angles in the same plane, thus, 
11–12	Water, . .	0·845	1129·8	11·5	588.5	157·4	Tube bent four times at right angles; at one point bent at an angle of about 135° to its former plane.
13–14	Water, . .	0·845	1129·8	11·4	588·5	161	Tube again bent as in experiments 10 and 11.
15–17	Water, . .	0·845	1129·8	33·0 C	588·5	108	

exhausted receiver, and the other was at a given instant immersed in water. The rate of flow having been determined, the tube was bent four times at right angles, and the experiment repeated; then it was not only bent four times at right angles *in one plane*, but bent at one point at an angle of about 135° to its former plane.

The results of these various experiments are exhibited in Table I., page 196.

It results from these experiments that the bends in the tubes had no perceptible influence in modifying the flow—the quantity of fluid flowing in the same time being directly as the pressure, and very much influenced by rises of temperature.

II.—*Rate of Flow of Defibrinated Blood of Sheep.*

Having determined that the shape of the tubes exerted no influence on the flow of fluids through them, we proceeded to examine the comparative rate of flow of the defibrinated blood of the sheep. The results are recorded in Table II.

The tube used in this experiment was 908·9 millimetres long, and was twice bent at right angles. The diameter was 1·214 millimetres.

TABLE II.

No. of Experiments.	Fluid used.	Diameter of Tube.	Length of Tube.	Temperature.	Pressure.	Rate of Flow of 100 Cubic Cents in Seconds.
		mm.	mm.		mm.	
18–21	Water, . .	1·214	908·9	10°·5	583·5	67·6
22–25	Defibrinated sheep's blood, . .	,,	,,	16·7	583·5	227·6
26–28	,,	,,	,,	,,	,,	
29–31	,,	,,	,,	,,	,,	
32–35	,,	,,	,,	31·0	,,	143.4

TABLE III.

Comparative Rate of Flow of Water, Defibrinated Ox-Blood, Serum of Blood (obtained from same sample of Blood), and Defibrinated Sheep's Blood.

No. of Experiments.	Fluid used.	Diameter of Tube.	Length of Tube.	Temperature.	Pressure.	Time occupied by Flow of 100 Cubic Cents in Seconds.
		mm.	mm.		mm.	
36	Water, . .	1·214	908·9	12°·0 C	598·7	68·16
37*	Serum of ox-blood,	,,	,,	13·1	,,	97·10
38	,,	,,	,,	,,	,,	98·14
38	,,	,,	,,	16°·0	,,	94·50
40	Defibrinated ox-blood, . .	,,	,,	,,	,,	365·7
41†	Defibrinated sheep's blood,	,,	,,	18°·0	,,	260·2

III.—*On the Rate of Flow of Pure* (i.e., *uncoagulated*) *Blood at the Temperature of Body through Narrow Tubes.*

Exp. 43.—In this experiment a calf, about a week old, was made use of. The jugular vein on the left side having been exposed, an opening was made into it as low in the neck as possible, and a flexible catheter was passed into the right side of the heart; the venous blood used was thus obtained.

Thereafter the carotid artery was exposed on the same side, and a ligature having been applied on the distal side of the exposed portion, a tube was introduced into the cardiac end. From this tube was obtained the arterial blood used in the experiment.

The temperature of the calf before the experiment was, 38°·8 C.
After the experiment, ... 38°·7 C.

The blood was received directly into graduated tubes heated to 38·°8 C.

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* Solids in 1000 parts of serum, ...	90·41
Water, ...	909·59
† Solids in 1000 parts of the blood, ...	212·21
Water, ...	787·79

Two tubes were used in these experiments. The length was 56 inches. The first (Tube C) had a diameter of 1·259 of a millimetre. The second (Tube A) had a diameter of 0·9289 of a millimetre.

TABLE IV.

No. of Experiments.	Fluid used.	Diameter of Tube.	Length of Tube.	Temperature.	Pressure.	Time of Flow of 100 Cubic Cents in Seconds.
		Tube C. mm.	mm.		mm.	
43	Water, . .	1·259	914·	15°· C	601·7	42·10
44	Water, . .	,,	,,	39°·5 C	,,	39·43
45, 46	Venous blood of calf,	,,	,,	38°·8	589·0	54·9
46, 47	Venous blood of calf, defibrinated and arterial, .	,,	,,	,,	,,	53·11
48, 49	Arterial blood of calf,	,,	,,	,,	,,	60·07
		Tube A.				
50	Water, . .	0.9289	914·	38°·5	601·7	69·4
15–53	Arterial blood of calf,	,,	,,	,,	,,	160·1

From this experiment it would appear that the rate of flow of blood just drawn from the vessels of a living animal is very much greater than the rate of flow of blood which, having been defibrinated, has been allowed to stand for some time, as was the case in experiment 40. In defibrinated blood the corpuscles tend undoubtedly to run together, and the masses thus formed by their coherence must necessarily account for the extreme slowness. The pure and perfectly warm blood flowed, indeed, more rapidly than did the serum obtained from ox-blood, which had been used in a previous experiment. In experiments 36, 37, 38, and 39, it was found that the time of flow of equal quantities of serum and water were represented by the ratio of 1·4 : 1. In experiments 43–49, it was found, on the other hand, that the rate of flow of equal quantities of pure blood and water were represented by the ratio of 1·3 : 1.

In a former part of this paper we stated that the diameters of the tubes used by us differed from those of Poiseuille in being much wider.

As was previously stated, the French author found that in capillary tubes of different diameter, the quantity of fluid flowing in equal times through equal lengths, varies not as the squares, but as the fourth power of the diameters. In the tubes used by us, in the experiment above described, the diameter was such that the quantities of water flowing through equal lengths were, *cæteris paribus*, as the squares of the diameters. It is interesting to observe in connection with experiments 43–53 inclusive, that whilst the amount of water flowing varied very much as the squares of the diameters, the quantity of blood flowing through the two tubes did not obey this law; the blood being retarded in its flow more than water though by no means to such an extent as to show that, for it, the tubes obeyed Poiseuille's law.

IV. *On the Pressure required to force Blood Clot through Tubes of Narrow Diameter.*

The clot used was obtained by allowing ox's blood to coagulate, and separating it from serum.

Exp. 54.—In this experiment a tube having a diameter of 1·162 millimetre was used. Although subjected to the whole atmospheric pressure (700 M.) none of the clot would pass through the tube.

Exp. 55 *and* 56.—In this experiment the same clot was used, but a different tube. The clot was found freely to flow through the tube, which had a diameter of 2·00 millimetres.

In experiment 55 the pressure of a column of mercury 24 inches high was employed. In experiment 56 that of a column 29 inches high was required.

V. *On the Rate of Flow of Milk and Urine through Narrow Tubes.*

The results of these experiments are shown in the annexed table. It will be observed that two tubes were employed in the determination of the rate of flow of milk, whilst the two sets of experiments with urine were performed with one tube. The rate of flow of urine is shown to be almost identical with that of water, whilst the rate of flow of milk is about the same as that of water when a large tube is used, but much slower when a tube of narrow diameter is employed.

TUBE A.

Fluid Used.	Diameter of Tube.	Length of Tube.	Temperature.	Pressure.	Time of Flow of 100 Cubic Cents in Seconds.
	mm.	mm.		mm.	
Water, . .	·928	914	17° C	601·97	69·2
Urine, Sp. Gr. 1018	,,	,,	17·5	,,	71·3
Urine, Sp. Gr. 1007	,,	,,	,,	,,	70·3
Cow's Milk, .	,,	,,	24·6	594·3	90·3

TUBE C.

Fluid Used.	Diameter of Tube.	Length of Tube.	Temperature.	Pressure.	Rate of Flow of 100 Cubic Cents in Seconds.
	mm.	mm.		mm.	
Water, . .	1·259	914	15°	601·97	42·1
Cow's Milk, .	,,	,,	27	,,	38·1
Goat's Milk, .	,,	,,	22	,,	36·09

8. On Cystine ($C_3H_7NO_2S$). By James Dewar, F.R.S.E., Lecturer on Chemistry, Veterinary College, Edinburgh; and Arthur Gamgee, M.D., F.R.S.E., Lecturer on Physiology, at Surgeon's Hall, Edinburgh.

Preliminary Notice.

With the exception of the physical characters of this rare chemical substance, which is only known as an abnormal constituent of the human body, we know so very little, that even a few facts with regard to its behaviour with reagents may not be altogether uninteresting.

Cystine has the composition $C_3H_7NO_2S$; and crystallises in the form of six-sided plates. It forms with hydrochloric, nitric, and phosphoric acids, definite crystalline compounds.

Some of the most important facts with regard to the chemical reactions of cystine have been recorded by Dr Bence Jones, who for the first time showed that nitrous acid decomposes it with the evolution of nitrogen, and that in this operation the sulphur which it contained is oxidised to sulphuric acid, whilst a non-crystalline substance is left which is precipitable by nitrate of silver, mercuric chloride, as well as by acetate of lead.

The cystine used in our experiments was obtained from two portions of calculi, one of which was furnished to us by Professor Maclagan, the other by the Royal College of Surgeons of Edinburgh. The cystine was obtained by treating the pounded calculi with strong liquor ammoniæ, which dissolved the greater part, and allowing the solution to evaporate at a very gentle heat. The cystine which separated was then again dissolved in ammonia and recrystallised.

Preparation of Hydrochlorate of Cystine.

One gramme of cystine was dissolved in boiling hydrochloric acid; on cooling beautiful needle-shaped crystals separated, which were very soluble in water. When thoroughly dried in vacuo over quicklime the crystals were found not to be readily soluble in water. 0·05 grm. of crystalline hydrochlorate of cystine yielded 0·0452 grm. of AgCl, corresponding to 22·2 per cent. of HCl (Calcd. 22·5).

Action of Nitrate of Silver on Cystine.

Cystine was dissolved in strong solution of ammonia, and to the solution was added a solution of silver nitrate in ammonia. No precipitate occurred, nor did the solution darken in the cold. When slightly acidified with nitric acid, a canary-yellow precipitate was thrown down, which was collected and dried *in vacuo*. The filtrate blackened when heated, and on filtering off the black precipitate a clear colourless solution was obtained, which was not further blackened when boiled with ammoniacal solution of oxide of silver.

On analysis the substance precipitated proved to be a compound of cystine with nitrate of silver.

In a subsequent experiment an ammoniacal solution of cystine was boiled with an ammoniacal solution of nitrate of silver. A black precipitate fell which consisted of sulphide of silver. The filtrate from the precipitate of sulphide of silver was subsequently treated with solution of chloride of ammonium to separate the excess of silver. The solution was found not to be precipitated by hydrochloric acid and chloride of barium, nor by sulphate of calcium. It is therefore evident that when an ammoniacal solution of cystine is heated with ammoniacal solution of oxide of silver, the sulphur is separated entirely as sulphide of silver, none being oxidised; it is also obvious that no oxalic acid is formed.

Action of Caustic Soda and Caustic Baryta on Cystine.

Cystine, when treated with pure solution of pure NaHO, and evaporated in a silver basin, gives a reddish fluid; sulphide of sodium is then produced, blackening the basin, and ammonia is copiously evolved. On treating the residue with water, neither sulphuric nor oxalic acids can be detected. The liquid contains, however, a large quantity of sulphide of sodium with a mere trace of sulphite.

Cystine, when heated to 150° C. with solution of caustic baryta in sealed tubes, gave off ammonia, a large quantity of sulphide of barium, a smaller quantity of sulphite of barium, and a trace of hyposulphite being formed. No trace of sulphocyanide could be detected.

Action of Alcoholic Solution of Potash on Cystine.

Cystine was heated for several hours in a sealed tube at 130° C with an alcoholic solution of potash. At the conclusion of the experiment a small quantity of dark sticky matter was found adhering to the tube, which contained a yellowish fluid. The latter smelt strongly of ammonia, which was separated by distillation. The residue was acidified with dilute sulphuric acid, and shaken up with ether. Ether left a yellow non-crystalline substance, possessed of an indefinite but disagreeable odour. This substance had a strong acid reaction, and was found to contain no sulphur.

Action of Nascent Hydrogen on Cystine.

When cystine is added to a mixture of tin or zinc and dilute hydrochloric acid, large quantities of sulphurated hydrogen are given off; the evolution of gas gradually slackens, till even after the action has gone on for several days, traces of sulphuretted hydrogen continue to be given off. When treated in the same manner taurine does not evolve H_2S.

It is to be noted that this evolution of H_2S, when cystine is treated with tin or zinc and hydrochloric acid, might be used as a test for the substance, care being previously taken to separate any sulphide which might exist.

Action of Nitrous Acid on Cystine.

Cystine was placed in water and a stream of nitrous acid gas passed through it. No action took place until the water was heated; it then commenced and proceeded briskly, with abundant effervescence, until the whole of the substance was dissolved.

The clear solution contained a large quantity of sulphuric acid, but not a trace of oxalic acid. When boiled with an ammoniacal solution of nitrate of silver, considerable reduction took place, a beautiful mirror of silver being deposited on the glass. The fluid was again subjected to the action of nitrous acid; still no oxalic acid could be found, and the reduction of an ammoniacal solution of oxide of silver continued. A portion of the fluid was treated with carbonate of barium and heated; the clear filtrate had an alkaline reaction, and was abundantly precipitated by nitrate of silver and acetate of lead. The remainder of the fluid, after the treatment with $BaCO_3$, was treated with solution of nitrate of silver. An abundant canary-yellow precipitate was formed. This was suspended in water and decomposed with H_2S; the filtrate was evaporated to dryness, and presented the appearance of a sticky solid. It was soluble in water. The aqueous solution was evaporated and treated with absolute ether, which dissolved the greater part. The ethereal solution left on evaporation an acid fluid. This was dissolved in water, neutralised with ammonia, and precipitated with solution of nitrate of silver. The yellow precipitate obtained was amorphous; it was dried in *vacuo*. Two specimens of the silver salt prepared at different times were analysed by us. The following are the results of two analyses:—

Silver, . . .	56·9	57·5
Carbon, . . .	19·43	21·32
Hydrogen, . .	5·29	4·64

In considering the discrepancies of these analyses, it must be borne in mind that we were operating in excessively small quantities of a substance prepared at different times by complicated processes.

Remarks.

Cramer believed that cystine was intimately related to the body called Serin, $C_3H_7NO_3$, which is obtained as one of the products of the action of alkalies on silk. Serin, when treated with nitrous acid, yields glyceric acid, as alanine under the same circumstances yields lactic acid, and therefore serin may be looked upon as amido-glyceric acid.

Cramer further believed that cystine was a sulpho-amido-glyceric acid, *i.e.*, serin in which hydroxyl has been replaced by HS.

This supposed relation is exhibited below—

CH_2OH	CH_2NH_2	CH_2NH_2
$CHOH$	$CHOH$	$CHSH$
CO_2H	CO_2H	CO_2H
Glyceric Acid.	Amido-glyceric Acid or Serin.	Cystine.

Considering that this relation of cystine to serin really exists, some have argued that on treatment with nitrous acid, cystine should yield glyceric acid. We do not, however, admit that this would really be the case. If we examine the case of sulpho-lactic acid, an analogous body to the supposed sulphur derivative of serin, we find that, on oxidation, it gives sulpho-propionic acid, and therefore we should, in the case of cystine, expect that a sulpho-acid would be formed on treatment with nitrous acid, were it built up as Cramer supposed. We have uniformly observed, during the course of our experiments, that, however carefully we attempted to regulate the action of nitrous acid on cystine, or of a nitrite on a salt of cystine, the sulphur separated as sulphuric acid thus pointing to a material difference in its reactions from what we should have expected from its supposed constitution. Although we cannot consider our experiments as definitive, we can assert that glyceric acid is not a product of the action of nitrous acid, and we venture to predict that, in all probability, cystine will be found to be related to pyruvic acid—to be an amido-sulpho-pyruvic acid. We base this supposition on the near approach of the analyses of the silver salt of the acid obtained by the action of nitrous acid on cystine, to the composition of a pyruvate, and on the general character of the oily acid produced.

We intend to pursue this subject further.

9. Notes from the Physical Laboratory of the University

By Professor Tait. (With a Plate.)

After passing through the usual routine work of acquiring skill in the fundamental operations, several of my students have remained long enough in the laboratory to make investigations in various branches of Physics. A great many of these inquiries related to matters already thoroughly known; but some have claims to notice as dealing with subjects on which our information is as yet incomplete. These I propose, from time to time, to lay before the Society. Their value as scientific results must depend on the skill and care of the experimenters. For the forms of apparatus employed, and the mode of conducting the experiments, I am, in most cases, responsible.

(1.) Mr J. P. Nichol has made a long series of experiments upon the Radiation and Convection of Heat, mainly to determine the amount of radiation in absolute measure, but incidentally with a view to finding how convection varies with the density of the air. The following is a preliminary notice of his work. The radiating body was a thin spherical shell of copper, filled with hot water. Its surface was sometimes bright, sometimes covered (by means of photographic varnish) with lamp-black. It was suspended by fine wires in a metallic vessel, which was blackened internally, fitted with a pressure-gauge, surrounded by cold water, and connected with an air-pump. An iron cup was let into the top of the shell, and contained a little mercury surrounding the bulb of a thermometer whose stem ascended in a glass tube which was inserted in the lid of the closed vessel. Considerable trouble was caused at first by the water leaking out of the shell when its temperature was high and the vacuum good—but in the later experiments this was entirely got over.

As it was suspected that a difference of thickness of the lamp-black coating might influence the amount of radiation, the mode of experimenting finally adopted was to alter the air pressure in the vessel from time to time; first, for instance, half an hour's cooling at 100^{mm}, then half an hour at 200^{mm}, then at 100^{mm}, and so on. But the portions of the curves of cooling thus found on separate days fitted well together into a single continuous line, as is seen in the corner of the diagram, where the dotted lines belong to one day's experiments, and the double lines to those of another day.

The numbers (H) given in the following table, which is formed from means of many experiments, and which is shown graphically in the diagram, express in grains the quantity of water which would be heated 1° Centigrade by the heat lost (by radiation and convection jointly) by one square inch of surface in an hour, its temperature being kept constant. With the apparatus employed, it was not easy to keep the pressure lower than 10^{mm}; but the curves for different pressures show that in this case the convection must be small, so that (roughly) we may take the numbers given for that pressure as representing the radiation alone.

	Blackened.		Bright.	
Pressure.	Temperature C.	H.	Temperature C.	H.
760^{mm}	61·2	6258	63·8	3537
	50·2	4875	57·1	3091
	41·6	3867	50·5	2637
	34·4	3082	44·8	2251
	27·3	2294	40·5	2013
	20·5	1629	34·2	1571
			29·6	1353
			23·3	996
			18·6	751
102^{mm}	62·5	4650	67·8	1763
	57·5	4150	61·1	1552
	53·2	3760	55·	1371
	47·5	3220	49·7	1220
	43·	2835	44·9	1082
	28·5	1755	40·8	960
10^{mm}	62·5	4236	65·	1390
	57·5	3847	60·	1273
	54·2	3593	50·	1025
	41·7	2600	40·	786
	37·5	2292	30·	563
	34·	2040	23·5	445
	27·5	1600		
	24·2	1400		

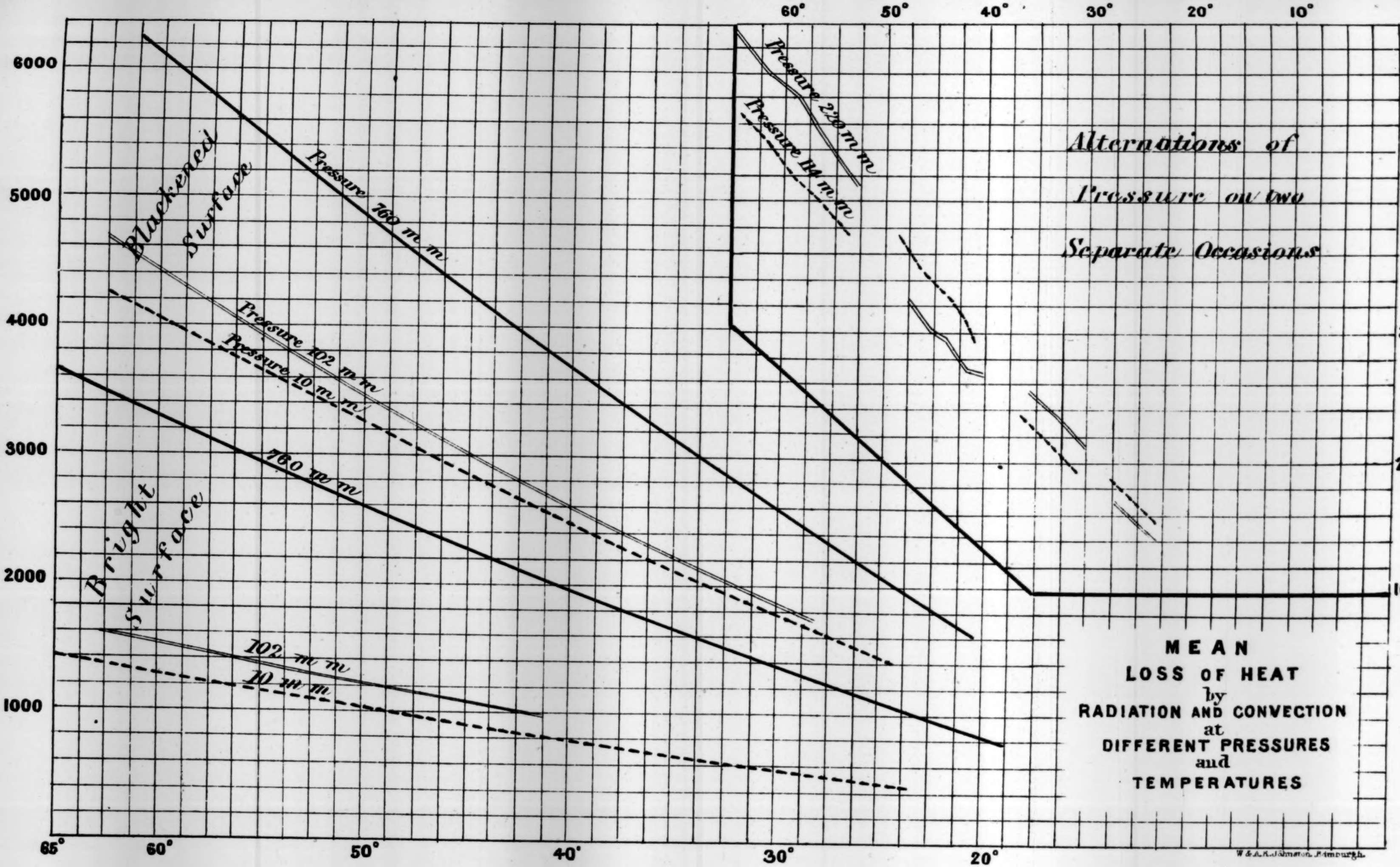
(2.) Mr A. Brebner made during last winter a number of careful determinations of the polarisation of electrodes of various materials in commercial sulphuric acid of various strengths and at various temperatures. The process employed was essentially the same as that described by me in the Proceedings R.S.E. for May 31, 1869. The following are means of many experiments:—

Platinum Electrodes.

Acid to Water.	Temperature C.	Polarisation.	Acid to Water.	Temperature C.	Polarisation.
1 : 50,	8°	224	1 : 10,	5°	188
	68°	187		56°	174
	87°	172		90°	157
1 : 40,	6°	178	1 : 0,	7°	202
	54°	169		66°	169
	84°	157		134°	133
1 : 20,	10°	174	1 : 0,	11°	244
	44°	167		102°	187
	88°	158		201°	137

The results of such experiments cannot be expected to be very accordant, but, if the means above given may be trusted, the polarisation is less for 1 acid to 20 water than for either stronger or weaker acids; and it also falls off more slowly with increase of temperature.

(3.) Messrs P. W. Meik and J. Murray made many observations with an electric balance, and resistance coils, to test the change of electric resistance produced in a wire by extension. The wires tested were of two specimens of copper—one of high, the other of very low, conducting power. They were taken of equal gauge and of such lengths as to have almost equal resistance; one was associated with a 10 B.A. Unit coil as one side of the balance, the other had associated with it a box of resistance coils initially set at 10 B.A.U. The value of the galvanometric scale was determined in each experiment by increasing by a small known amount the resistance of the coils in circuit. The results are not yet quite reduced; as we require to know the linear extension, and (if possible), the cubical contraction, of each wire produced by the appended weights. But, even in their present state, they appear to be of some consequence, as they show changes of conducting power almost exactly proportional to the weights appended, but singularly differing in absolute amount for these dissimilar specimens of copper.



MEAN
LOSS OF HEAT
by
RADIATION AND CONVECTION
at
DIFFERENT PRESSURES
and
TEMPERATURES

The following Donations to the Society were announced :—

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